PHILOSOPHICAL TRANSACTIONS,

OF THE

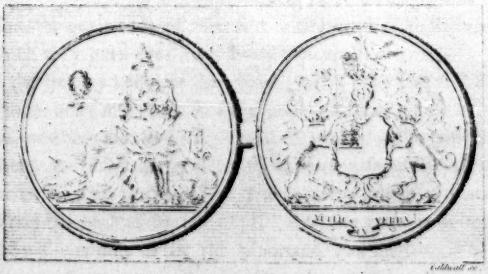
ROYAL SOCIETY

OF

LONDON.

VOL. LXXIII. For the Year 1783.

PART I.



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TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR 1783.

PART II.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

CONTENTS

OF

VOL. LXXIII. PART I.

I. *A LETTER from* William Herſchel, *Eſq. F. R. S.* page 1

II. *On the Diameter and Magnitude of the* Georgium Sidus; *with a Deſcription of the dark and lucid Diſk and Periphery Micrometers. By* William Herſchel, *Eſq. F. R. S.* p. 4

III. *Concluſion of the Experiments and Obſervations concerning the Attractive Powers of the Mineral Acids. By* Richard Kirwan, *Eſq. F. R. S.* p. 15

IV. *A Deſcription of a Species of Sareocele of a moſt aſtoniſhing Size in a Black Man in the Iſland of* Senegal; *with ſome Account of its being an endemial Diſeaſe in the Country of* Galam. *By* J. P. Schotte, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.* p. 85

V. *A Deſcription of a new Conſtruction of Eye-glaſſes for ſuch Teleſcopes as may be applied to Mathematical Inſtruments. By*

By Mr. Ramſden; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.* p. 94

VI. *Account of ſeveral Lunar Iris. By* Marmaduke Tunſtall, *Eſq. F. R. S. in Two Letters to Sir* Joſeph Banks, *Bart. P. R. S.* p. 100

VII. *Account of an Earthquake. By* John Lloyd, *Eſq. in a Letter to Sir* Joſeph Banks, *Bart P. R. S.* p. 104

VIII. *An Account of a new Eudiometer. By Mr.* Cavendiſh, *F. R. S.* p. 106

IX. *Experiments upon the Reſiſtance of the Air. By* Richard Lovell Edgworth, *Eſq. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.* p. 136

X. *An Anſwer to the Objections ſtated by M.* De la Lande, *in the Memoirs of the* French *Academy for the Year* 1776, *againſt the Solar Spots being Excavations in the luminous Matter of the Sun, together with a ſhort Examination of the Views aſcertained by him upon that Subject. By* Alexander Wilſon, *M. D. Profeſſor of Practical Aſtronomy in the Univerſity of* Glaſgow; *communicated by* Nevil Maſkelyne, *D D. F. R. S. and Aſtronomer Royal.* p. 144

XI. *An Account of the Earthquakes which happened in* Italy, *from* February *to* May 1783. *By Sir* William Hamilton, *Knight of the Bath, F. R. S.; in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.* p 169

XII. *Account of the Earthquake which happened* March 28, 1783. *In a Letter from Count* Franceſco Ippolito *to Sir* William Hamilton, *Knight of the Bath, F. R. S.; preſented by Sir* William Hamilton. p. 117

XIII. *Account of the* Black Canker Caterpillar, *which deſtroys the Turnips in* Norfolk. *By* William Marſhall, *Eſq. in a Letter to* Charles Morton, *M. D. F. R. S.* p. 217

XIV. *A Letetr from Mr.* Edward Nairne, *F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S. containing an Account of Wire being ſhortened by Lightning.* p. 223

XV. *An Account of Ambergriſe. By Dr.* Schwediawer; *preſented by Sir* Joſeph Banks, *Bart. P. R. S.* p. 226

XVI. *Extract of a Regiſter of the Barometer, Thermometer, and Rain, kept at* Lyndon, *in* Rutland, 1782. *By* Thomas Barker, *Eſquire.* p. 242

APPENDIX.

I. *Tranſlation of Count* Franceſco Ippolito's *Letter to Sir* William Hamilton, *Knight of the Bath, F. R. S.; giving an Account of the Earthquake which happened in* Calabria, March 28, 1783. p. i

PHILOSOPHICAL TRANSACTIONS.

I. *A Letter from* William Herſchel, *Eſq. F. R. S.*

TO SIR JOSEPH BANKS, BART. P. R. S.

SIR,

BY the obſervations of the moſt eminent Aſtronomers in Europe it appears, that the new ſtar, which I had the honour of pointing out to them in March, 1781, is a Primary Planet of our Solar Syſtem. A body ſo nearly related to us by its ſimilar condition and ſituation, in the unbounded expanſe of the ſtarry heavens, muſt often be the ſubject of the converſation, not only of aſtronomers, but of every lover of ſcience in general. This conſideration then makes it neceſſary to give it a name, whereby it may be diſtinguiſhed from the reſt of the planets and fixed ſtars.

In the fabulous ages of ancient times the appellations of Mercury, Venus, Mars, Jupiter, and Saturn, were given to the Planets, as being the names of their principal heroes and divinities*. In the preſent more philoſophical æra, it would hardly be allowable to have recourſe to the ſame method, and call on Juno, Pallas, Apollo, or Minerva, for a name to our new heavenly body. The firſt conſideration in any particular event, or remarkable incident, ſeems to be its chronology: if in any future age it ſhould be aſked, *when* this laſt-found Planet was diſcovered? It would be a very ſatisfactory anſwer to ſay, "In the Reign of King George the Third." As a philoſopher then, the name of GEORGIUM SIDUS preſents itſelf to me, as an appellation which will conveniently convey the information of the time and country where and when it was brought to view. But as a ſubject of the beſt of Kings, who is the liberal protector of every art and ſcience;—as a native of the country from whence this Illuſtrious Family was called to the Britiſh throne;—as a member of that Society, which flouriſhes by the diſtinguiſhed liberality of its Royal Patron;—and, laſt of all, as a perſon now more immediately under the protection of this excellent Monarch, and owing every thing to His unlimited bounty;—I cannot but wiſh to take this opportunity of expreſſing my ſenſe of gratitude, by giving the name Georgium Sidus,

Georgium Sidus
——*jam nunc aſſueſce vocari.* VIRG. Georg.

to a ſtar, which (with reſpect to us) firſt began to ſhine under His auſpicious reign.

* M. DE LA LANDE's Aſt. §. 639.

By

By addreſſing this letter to you, SIR, as Preſident of the Royal Society, I take the moſt effectual method of communicating that name to the Literati of Europe, which I hope they will receive with pleaſure. I have the honour to be, with the greateſt reſpect,

SIR,

Your moſt humble

and moſt obedient ſervant,

W. HERSCHEL.

II. *On the Diameter and Magnitude of the* Georgium Sidus; *with a Deſcription of the dark and lucid Diſk and Periphery Micrometers. By* William Herſchel, *Eſq. F. R. S.*

Read November 7, 1782.

IT is not only of the greateſt conſequence to the aſtronomer, but alſo gives the higheſt pleaſure to every intelligent perſon, to have a juſt idea of the dimenſions of the ſolar ſyſtem, and the heavenly bodies that belong to it. As far then as they fall within the reach of our inſtruments, they ought carefully to be examined and meaſured by all the various methods we can invent. Almoſt every ſort of micrometer is liable to ſome inconveniences and deceptions: it will, however, often happen, that we may correct the errors of one inſtrument by the oppoſite defects of another. The meaſures of the diameter of the Georgium Sidus, which were delivered in my firſt paper, differ conſiderably from each other. However, if we ſet aſide the three firſt, on a ſuppoſition (as I have hinted before) that every minute object, which is much ſmaller than what we are frequently uſed to ſee, will at firſt ſight appear leſs than it really is; and take a mean of the remaining obſervations, we ſhall have

have 4″ 36‖‴ for the diameter of the planet. On comparing the meaſures then with this mean, we find but two of them that differ ſomewhat more than half a ſecond from it; the reſt are almoſt all within a quarter of a ſecond of that meaſure. This agreement, in the dimenſions of any other planet, would appear very conſiderable; but not being ſatisfied, when I thought it poſſible to obtain much more accurate meaſures, I employed the lamp-micrometer in preference to the former. The firſt time I uſed it upon this occaſion I perceived, that if, inſtead of two lucid points, we could have an intire lucid diſk to reſemble the Planet, the meaſures would certainly be ſtill more compleat. The difficulty of dilating and contracting a figure that ſhould always remain a circle, appeared to me very conſiderable, though nature, with her uſual ſimplicity, holds out to us a pattern in the Iris of the eye, which, ſimple as it appears, is not one of the leaſt admirable of her inimitable works. However, I recollected, that it was not abſolutely requiſite to have every inſenſible degree of magnitude; ſince, by changing the diſtance, I could without much inconvenience make every little intermediate gradation between a ſet of circles of a proper ſize, that might be prepared for the purpoſe. Intending to put this deſign into practice, I contrived the following apparatus.

A large lanthorn, of the conſtruction of thoſe ſmall ones that are uſed with my lamp-micrometer *, muſt have a place for three flames in the middle, which is neceſſary, in order that we may have the quantity of light required, by lighting one, two, or all of them. The grooves, inſtead of braſs ſliding doors, muſt be wide enough to admit a paſte-board, and three or four thickneſſes of paper. I prepared a ſet of circles, cut

* Phil. Tranſ. vol. LXXII. p. 166.

out

out in paſte-board, increaſing by tenths of an inch from two inches to five in diameter, and theſe were made to fit into the grooves of the lamp. A good number of pieces, ſome of white, others of light blue paper, of the ſame ſize with the paſte-boards, were alſo cut out, and ſeveral of them oiled, to render them more tranſparent. The oiled papers ſhould be well rubbed, that they may not ſtain the dry papers when placed together. This apparatus being ready, we are to place behind the paſte-board circle, next to the light, one, two, or more, either blue or white, dry or oiled, papers; and by means of one or more flames, to obtain an appearance perfectly reſembling the diſk we would compare it with. It will be found, that more or leſs altitude of the object, and higher or lower powers of the inſtrument, require a different aſſortment of papers and lights, which muſt by no means be neglected: for if any fallacy can be ſuſpected in the uſe of this apparatus, it is in the degree of light we muſt look for it. In a few experiments I tried with theſe lucid diſks, where I placed ſeveral of them together, and illuminated them at once, it was found, that but very little more light will make a circle appear of the ſame ſize with another, which is one, or even two-tenths of an inch leſs in diameter. A well known and ſtriking inſtance of this kind of deception is the moon, juſt before or after the conjunction, where we may ſee how much the luminous part of the diſk projects above the reſt.

The method of uſing the artificial diſks is the ſame which has been deſcribed with the lamp-micrometer, of which this apparatus may be called a branch. We are only to obſerve, that the Planet we would meaſure ſhould be cauſed to go either juſt under, or juſt over, the illuminated circle. It may indeed alſo

also be suffered to pass across it; but in this case, the lights will be so blended together, that we cannot easily form a proper judgement of their magnitudes. By a good screw to the motions of my telescope I have been able, at any time, to keep the Planet opposite the lucid disk for five minutes together, and to view them both with the most perfect and undisturbed attention. The apparatus I employed being now sufficiently explained, several alterations that were occasionally introduced will be mentioned in the observations and experiments on the Georgium Sidus, as they follow, in the order of time in which they were made.

Observations on the Light, Diameter, and Magnitude, of the Georgium Sidus.

Oct. 22, 1781. The Georgium Sidus was perfectly defined with a power of 227; had a fine, bright, steady light; of the colour of Jupiter, or approaching to the light of the Moon.

Nov. 28, 1781. I measured the diameter of the Georgium Sidus by the lamp-micrometer, and took one measure, which I was assured was too large; and one, which I was certain was too little; then taking the mean of both, I compared it with the diameter of the star, and found it to agree very well.

Hence $\frac{\text{Image} = 2{,}4 \text{ inches}}{\text{Distance} = 431 \text{ inches}}$ = tang. ,0055684; and $\frac{\text{Angle} = 19' 8''}{\text{Power} = 227\ 6}$ = the diameter $5''{,}06$. But the evening was foggy, and the star having much aberration, I was induced to try the above method of extreme and mean diameters, suggested by the

the method of altitudes, where two equally distant extremes give us a true mean.

Nov. 19, 1781. The diameter measured 32¼ parts of my micrometer, the wires being outward tangents to the disk. On shutting them gradually by the same light, they closed at 24; therefore the difference is 8¼ parts, which, according to my scale, gives 5″ 2‴ for the diameter. This was taken with 227, and the measure seemed large enough. Not perfectly pleased with my light, which was rather too strong, I repeated the measure, and had 33¼ parts; then shutting the wires gradually, by *this* light they closed at 25: the difference, which is 8¼ parts, gives 5″ 11‴.

Aug. 29, 1782. 15 h. I saw the Georgium Sidus full as well defined with 460, as Jupiter would have been at that altitude with the same power.

Sept. 9, 1782. Circumstances being favourable, I took a measure of the diameter of the Georgium Sidus with the power of 460, and silk-thread micrometer. After a proper allowance for the zero, I found 4″ 11‴.

Oct. 2, 1782. I had prepared an apparatus of lucid disks, and measured the diameter of the Georgium Sidus with it. Having only white oiled papers, I placed two of them together, and used only a single lamp; but could not exactly imitate the light of the Planet. When I first saw the Sidus and luminous circle together, I was struck with the different colours of their lights; which brought to my recollection γ Andromedæ, ε Bootis, α Herculis, β Cygni, and other coloured stars. The Planet unexpectedly appeared blueish, while the lucid disk had a strong tincture of red; but neither of the colours were so vivid and sparkling as those of the just mentioned stars.

The diſtance of the luminous circle from the eye (which I always meaſure with deal rods) was 588,25 inches. The circle meaſured 2,35 inches. Hence we have the angle 13′ 44″; which, divided by the power 227, gives 3″,63 for the diameter of the Planet. I ſuſpected ſome little fallacy from the want of a perfect reſemblance in the light and colour of the artificial diſk to the real appearance of the Planet.

Oct. 4, 1782. I meaſured the diameter of the Georgium Sidus again, by an improvement in my apparatus, for I now uſed pale blue papers, both oiled and plain, inſtead of white; by which means I obtained a reſemblance of colours; and by an aſſortment of one oiled and two dry papers with two lamps burning, I effected the ſame degree of light which the Planet had, and both figures were equally well defined. By firſt changing the diſk, and, when I had one which came neareſt, changing my diſtance, I came at a perfect equality between the Planet and diſk. The meaſure was ſeveral times repeated with great precaution. The reſult was $\frac{2,8}{692,6} =$,0040283; and $\frac{13' 53'',85}{227} = 3'',67$. If any thing be wanting to the perfection of this meaſure, it is perhaps that the Sidus ſhould be in the meridian, in order to have all the advantages of light and diſtinctneſs.

Oct. 10, 1782. The meaſures of the Planet by the lucid diſk micrometer appearing to me very ſmall, I reſolved to aſcertain the power of my teleſcope again moſt ſcrupulouſly, by an actual experiment, without any deduction from other principles. On a moſt convenient and level plain I viewed two ſlips of white paper, and meaſured their images upon a wall. The diſtances were meaſured by deal rods, every repetition whereof

was certainly true to half a tenth of an inch; nor did the direction of the meaſure ever deviate, ſo much as two inches, from a ſtraight line.

Diſtance of the object from the eye in inches	7255,5
Diſtance of the eye from the vertex of the ſpeculum	80,2
Diſtance of the vertex of the ſpeculum from the object	7335,7
Diſtance of the eye from the wall	2292,35
Diameter of the largeſt paper	,99125
Diameter of the ſmalleſt	,5075
Image of the largeſt paper on the wall	73,
Image of the ſmalleſt on the ſame	37,8

Angle ſubtended by the large paper at the vertex of the ſpeculum 27″,87

Angle ſubtended by its image on the wall, at the eye 1° 49′ 26″,4.

Power of the teleſcope deduced from the large paper 235,6

Angle ſubtended by the ſmall paper at the vertex of the ſpeculum 14″,27.

Angle ſubtended by its image on the wall, at the eye, 56′ 40″,9,

Power of the teleſcope deduced from the ſmall paper 238,3

Mean of both experiments, as being equally good 237,

Focal length of the ſpeculum upon thoſe objects	86,1625
Upon Capella	85,2

And 237 diminiſhed in the ratio of 85,2 to 86,1625 gives 234,3 for the power of the inſtrument upon the fixed ſtars.

It

It appears then, from these experiments, that the power of the telescope has not been over-rated; and that, therefore, the measures of the Georgium Sidus cannot be found too small on that account.

There is one cause of inaccuracy or deception in very small measures, long suspected, but never yet sufficiently investigated. That there is a *dispersion* of the rays of light in their passage through the atmosphere, we may admit from various experiments; if then the quantity of this dispersion be, in general, regulated by certain dispositions of the air, and other causes, it will follow, that a *concentration* may also take place: for should the rays of light, at any time, be less dispersed than usual, they might with as much reason be said to be concentrated, as the mercury of a thermometer is said to be contracted by cold, when it falls below the zero.

Oct. 12, 1782. The night was so fine, that I saw the Georgium Sidus very plainly with my naked eye. I took a measure of its diameter by the lucid disk, and found, that I was obliged to come nearer, as the Planet rose higher, and gained more distinct light. At the altitude of 52° it was as follows:

$\frac{3,415}{731,3} = ,0046698$; and $\frac{16'3'',2}{227} = 4'',24$.

Oct. 13, 1782. 16 h. I viewed the Georgium Sidus with several powers. With 227 it was beautiful. Still better with 278. With 460, after looking some time, very distinct. I perceived no flattening of the polar regions, to denote a diurnal motion; though, I believe, if it had had as much as Jupiter, I should have seen it. With 625 pretty well defined.

Oct. 19, 1782. The inconvenience arising from the quantity of light contained in the lucid disk, suggested to me the

idea of taking only an illuminated periphery, inſtead of the area of a circle. By this means I hoped to ſee the circle well defined, and yet have but little light to interfere with the appearance of the Planet. The breadth of my lucid periphery was one-twentieth of an inch. The reſult of this meaſure proved $\frac{3,3}{765,45} = ,0041486$; and $\frac{14'\ 15'',69}{227} = 3''77$.

Oct. 26, 1782. In my laſt experiment I found the lucid periphery much broader than I could have wiſhed; therefore, I prepared one of no more than one-fortieth part of an inch in breadth, the outer circle meaſuring very exactly 4,00, and the inner circle 3,95. With this ſlender ring of light illuminated with only one ſingle lamp, I meaſured the Georgium Sidus, by removing the teleſcope to various diſtances; and found at laſt the following reſult: $\frac{4.}{1033,05} = ,0038720$; and $\frac{13'\ 18'',6}{227} = 3'',51$.

Nov. 4, 1782. I was now fully convinced that light, be it in the form of a lucid circle, or illuminated periphery, would always occaſion the meaſures to be leſs than they ſhould be, on account of its vivid impreſſion upon the eye, whereby the magnitude of the object, to which the Planet was compared, would be increaſed. It occurred to me then, that if a lucid circle encroached upon the ſurrounding darker parts, a lucid ſquare border, round a dark circle, would in its turn advance upon the artificial diſk. In my laſt meaſures, where the Planet had been compared to a lucid ring, I had plainly obſerved that the Sidus, which was but juſt equal to the illuminated periphery, was conſiderably larger than the black area contained within the ring. This ſeemed to point out a method to diſcover the quantity of the deception ariſing from the illumination; and conſequently, to furniſh us with a correction applicable to ſuch meaſures;

ſures; which would be *plus*, when taken with a lucid diſk or ring; and *minus*, when obtained from a dark ring or circle. Having ſuſpended a row of paſte-board circles againſt an illuminated ſheet of oiled paper, I cauſed the Georgium Sidus to paſs by them ſeveral times, and ſelected from their number that to which the Planet bore the greateſt reſemblance in magnitude. I produced a perfect equality by ſome ſmall alteration of my diſtance, and the reſult was as follows:

$$\frac{3,165}{633,95} = ,0049925: \text{ hence } \frac{17'\,9'',8}{227} = 4'',53.$$

I was deſirous of ſeeing what would be the effect of leſſening the light of the illuminated frame, againſt which the dark diſks were ſuſpended, and alſo waited a ſhort time that the Planet might riſe up higher. The meaſure being then repeated at a different diſtance, and with a different black diſk, I obtained the following particulars:

$$\frac{3,59}{803,05} = ,0044704; \text{ and } \frac{15'\,22'',1}{227} = 4'',06.$$

I intend to purſue theſe experiments ſtill farther, eſpecially in the time of the Planet's oppoſition, and am therefore unwilling as yet to draw a final concluſion from the ſeveral meaſures. In a ſubject of ſuch delicacy we cannot have too many facts to regulate our judgement. Thus much, however, we may in general ſurmiſe, that the diameter of the Georgium Sidus cannot well be much leſs, nor perhaps much larger, than about four ſeconds. From this, if we will anticipate more exact calculations hereafter to be made, we may gather that the real diameter of that planet muſt be between four and five times that of the earth: for by the calculations of M. DE LA LANDE, contained in a letter he has favoured me with, the diſtance of the Georgium Sidus is ſtated at 18,913, that of the earth

earth being 1. And if we take the latter to be ſeen, at the ſun, under an angle of 17″, it would ſubtend no more than ,″898, when removed to the orbit of the Georgium Sidus. Hence we obtain $\frac{4}{,898} = 4,454$; which number expreſſes how much the real diameter of the Georgium Sidus exceeds that of the earth.

III. *Concluſion of the Experiments and Obſervations concerning the Attractive Powers of the Mineral Acids.* *By* Richard Kirwan, *Eſq. F. R. S.*

Read Dec. 12, 1782.

HAVING found, as exactly as I was able, the quantity of each of the mineral acids taken up at the point of ſaturation by alkalies and earths, and alſo that taken up by phlogiſton, when theſe acids are by it converted into an aërial form, I next endeavoured to find how much of theſe acids was taken up at the point of ſaturation by each of the metallic ſubſtances, and for this purpoſe procured the moſt ſaturated ſolution poſſible of each metallic ſubſtance ſoluble in any of theſe acids. Theſe ſolutions did not, indeed, immediately anſwer my purpoſe, as they conſtantly retained an exceſs of acid; yet as they were the foundation of my ſubſequent obſervations, and as the experiments themſelves are in many reſpects uſeful to be known, I ſhall here briefly relate their reſult, and confine myſelf to thoſe circumſtances ſingly that relate to my future inveſtigations, or that have not heretofore been ſatisfactorily explained. The acids I uſed were dephlogiſticated ſo far as to be colourleſs; the metals were for the moſt part very fine filings, or reduced in a mortar to a fine powder. They were added little by little to their reſpective menſtruums, much more being thus diſſolved than if the whole was thrown in at once; and the ſolution was performed in glaſs phials with bent tubes.

Solution

Solution of iron in the vitriolic acid.

100 grs. of bar-iron, in the temperature of 56°, require for their ſolution, 190 grs. of real acid, whoſe proportion to that of the water with which it ſhould be diluted, is as 1 to 8, 10, or 12. It would act on iron, though its proportion were greater or leſſer, but not ſo vigouroufly. If towards the end a heat of 200° were applied, 123 grs. of real acid would be ſufficient.

The air produced by this ſolution is intirely inflammable, and generally amounts to 155 cubic inches.

Iron is alſo ſoluble with the aſſiſtance of a ſtrong heat, and in ſmaller quantity in concentrated vitriolic acid; and in this caſe ſcarce any inflammable air is produced, but a large quantity of vitriolic air as Dr. PRIESTLEY has obſerved, and a ſmall quantity of ſulphur ſublimes at the latter end. This fact is a clear refutation of Mr. LAVOISIER's hypotheſis; for is it not evident, that the ſame ſubſtance which, when a dilute acid is uſed, goes off in the form of inflammable air, does when a concentrated acid is uſed, to unite this acid, and thus form both vitriolic air and ſulphur? In the firſt caſe it cannot unite to the acid, by reaſon of the large quantity of water combined with the acid; and as the liquor, being moſtly aqueous, contains a large quantity of ſpecific fire, it receives that fire when the acid unites to the metallic earth, and flies off in the form of air. But in the ſecond caſe, the concentrated acid, containing much leſs ſpecific fire, cannot expel the phlogiſton in the form of inflammable air (as this air abſorbs a vaſt quantity of fire) but unites to it, when by heat it is further ſtripped of its water, and thus forms both

vitriolic air and ſulphur. 100 grs. of iron, diſſolved without heat, afford upwards of 400 of vitriol.

100 grs. of the vitriol cryſtallized contain 25 of iron, 20 of real acid, and 55 of water. When calcined nearly to redneſs theſe cryſtals loſe about 40 of water.

The calces of iron are more or leſs ſoluble in this acid according to their degree of dephlogiſtication. Thoſe that are phlogiſticated (as that recently precipitated from a ſolution of vitriol by fixed alkalies) are alſo moſt ſoluble, and upon evaporation afford cryſtals, though paler than thoſe formed of genuine iron. Thoſe that are leaſt phlogiſticated are alſo leaſt ſoluble, that is, require more real acid for their ſolution, and afford no cryſtals, but only a magma or mother liquor. Hence alſo, ſolutions of iron newly made diminiſh, and conſequently phlogiſticate the ſuper-incumbent air, and conſequently gradually emit phlogiſton: and hence the calx, being more dephlogiſticated, gradually falls unleſs more acid be added to keep it in ſolution.

Iron in the nitrous acid.

100 grs. of iron, to be perfectly diſſolved, and not barely calcined, require 142 grs. of real nitrous acid, ſo diluted as that its proportion to water ſhould be as 1 to 13 or 14; and when this laſt proportion is uſed, the heat of a candle may be applied for a few ſeconds, and the acceſs of common air prevented. In this caſe not above 18 cubic inches of nitrous air are produced, all the reſt is abſorbed by the ſolution, and no red vapours appear. But if the proportion of acid and water be as 1 to 8 or 10, and heat be applied, a much greater quantity of iron will be dephlogiſticated, though very little of it be held in ſolution; and by this means I have obtained from 100 grs. of iron, 83,87 cubic inches

of nitrous air; and by diſtilling the ſolution a ſtill greater quantity may be obtained, which was abſorbed by the ſolution. No inflammable air is obtained from ſolutions of iron or any other metallic ſubſtance in the nitrous acid, becauſe this acid has leſs affinity to water, and more to phlogiſton, than the vitriolic acid, and alſo contains much leſs fire than either the vitriolic or marine acids, as will be ſeen in the ſequel, and therefore unites to phlogiſton inſtead of barely expelling it. And hence it is, that the vitriolic acid, though united with 30 times its weight of water, will ſtill viſibly act on iron, and ſeparate inflammable air in the temperature of 55°; whereas nitrous acid, diluted with 15 times its weight of water, will have no viſible effect on iron in that temperature.

The calces of iron, if not too much dephlogiſticated, are alſo ſoluble, though difficultly, in the nitrous acid.

Iron in the marine acid.

100 grs. of iron require 215 of real marine acid for their ſolution. The proportion of acid to that of water in the ſpirit of ſalt I uſed was as 1 to 4. When it is as 1 to 4, it effervesces too violently. Heat is rather prejudicial, as it volatiliſes the acid. No marine air flies off, and the quantity of inflammable air is juſt the ſame as if dilute vitriolic acid were uſed.

The calces of iron are alſo ſoluble in marine acid. They may be diſtinguiſhed from genuine iron in this, that their colour, when precipitated by fixed alkalies, is *reddiſh*, whereas the precipitate of genuine iron is *greeniſh*.

Copper in the vitriolic acid.

100 grs. of copper require nearly 183 grs. real vitriolic acid for their ſolution. The proportion of acid to that of water

being as 1 to $\frac{1}{4}$, or at leaſt as 1 to $\frac{7}{10}$, and a ſtrong heat muſt alſo be applied. I could never diſſolve the whole of any quantity of copper; but to diſſolve a given quantity of it, a ſtill greater muſt be uſed in the proportion of nearly 28 to 100, though this reſiduum alſo is ſoluble by adding more acid. When copper has been dephlogiſticated in this manner, a ſolution of it is obtained by adding warm water to the dephlogiſticated maſs.

The dephlogiſtication of 128 grs. of copper treated in this manner affords 11 cubic inches of inflammable air, and nearly 65 of vitriolic air. When I obtained inflammable the acid was a little more aqueous. The reaſon why copper cannot be dephlogiſticated by dilute vitriolic acid, nor even by the concentrated, without the aſſiſtance of a ſtrong heat, as iron is, appears deriveable from its much ſtronger attraction to phlogiſton, and the much greater quantity of it which copper contains, as will hereafter be ſeen. Hence

100 grs. of vitriol of copper contain 27 of copper, 30 of acid, and 43 of water, of which it loſes about 28 by evaporation or ſlight calcination.

The ſolution of 100 grs. of copper affords 373 of blue vitriol.

Copper in nitrous acid.

100 grs. of copper require 130 of real nitrous acid to diſſolve them. If the acid be ſo far diluted as that its proportion to that of water be as 1 to 14, the aſſiſtance of heat will be neceſſary, otherwiſe not. This ſolution affords 67½ cubic inches of nitrous air.

The calces of copper are alſo ſoluble in this acid.

Copper in marine acid.

100 grs. of copper require 1190 grs. of real marine acid to diſſolve them, and alſo the aſſiſtance of a moderate heat, the proportion of acid to that of water being as 1 to $4\frac{1}{2}$, that is, its ſpecific gravity being 1,186, if a greater heat be uſed, more of the acid will be requiſite, as much will be diſſipated. If the acid be more concentrated, it will act more vigorouſly.

In my laſt paper I mentioned, that $8\frac{1}{2}$ grs. of copper afforded 86 of marine air: however, I muſt now add a circumſtance which I then did not attend to, which is, that the mercury over which that air was received was acted upon, ſo that part of the air was due to this action, which invalidates the concluſion I there drew concerning the quantity of phlogiſton in marine air, which hence appears to be greater than I there eſtimated it.

The calces of copper are alſo ſoluble in this acid, though not ſo eaſily as in the nitrous acid.

Tin in the vitriolic acid.

100 grs. of tin require for their perfect ſolution 872 grs. of real vitriolic acid, whoſe proportion to water ſhould not be leſs than as 1 to $\frac{9}{10}$, and alſo the aſſiſtance of a ſtrong heat; when the action of the acid has ceaſed, ſome hot water ſhould be added to the turbid ſolution, and the whole again heated. This ſolution affords 70 cubic inches of inflammable air. Tin is alſo ſoluble in a more dilute acid, but not in ſo great quantity.

The calces of tin (except that precipitated from marine acid by fixed alkalies) are inſoluble in this acid.

Tin in the nitrous acid.

100 grs. of tin require, for their perfect ſolution, 1200 grs. of real nitrous acid, whoſe proportion to water ſhould be at leaſt as 1 to 25, and the heat not exceeding 60°: the quantity of air afforded by ſuch ſolution is only ten cubic inches, and it is not nitrous. The ſolution is not permanent; for in a few days it depoſits a whitiſh calx, and if the weather be warm burſts the phial. The calces of tin are inſoluble in this acid.

Tin in the marine acid.

100 grs. of tin require for their ſolution 413 of real marine acid, whoſe proportion to water is as 1 to 4½, and alſo the aſſiſtance of a moderate heat. This ſolution affords about 90 cubic inches of inflammable air and 10 of marine air. The calces of tin are nearly inſoluble in this acid.

Lead in the vitriolic acid.

100 grs. of lead require for their ſolution 600 grs. of real acid, whoſe proportion to water is not leſs than that of 1 to $\frac{7}{10}$, and better if the quantity of water be ſtill leſs; and hence, as with regard to copper, a greater quantity of lead ſhould be employed than is expected to be diſſolved. A ſtrong heat is alſo requiſite, and hot water ſhould be added to the calcined maſs, though ſparingly, as it occaſions ſome precipitation.

This metal is alſo ſoluble, but in a very ſmall degree, in dilute vitriolic acid; for it effervesces with ſpirit of vitriol whoſe ſpecific gravity is only 1,275.

The

The calces of lead are ſomething more ſoluble in this acid. 100 grs. of vitriol of lead, formed by precipitation, contains 73 of lead, 17 of real acid, and 10 of water. Vitriol of lead, formed by direct ſolution, contains a large proportion of acid.

Lead in the nitrous acid.

100 grs. of lead require for their ſolution about 78 grs. of real acid, whoſe proportion to that of water may be as 1 to 11 or 12, and the aſſiſtance of heat towards the end. This ſolution affords but eight cubic inches of nitrous air. The calces of lead are alſo ſoluble in this acid; but if much dephlogiſticated they become leſs ſoluble.

100 grs. of minium require 81 grs. of real acid.

100 grs. of nitrous ſalt of lead contain about 60 of lead.

Lead in the marine acid.

100 grs. of lead require 600 grs. of real acid to diſſolve them, when the ſpecific gravity of the ſpirit of ſalt is 1,141, and alſo the aſſiſtance of heat, by which much of the acid is diſſipated. A ſtronger acid would diſſolve more.

The calces of lead are more ſoluble in this acid than genuine lead. 100 grs. minium require 327 grs. of real acid; but white lead is much leſs ſoluble.

100 grs. of horn lead, formed by precipitation, contain 72 of lead, 18 of marine acid, and 10 of water.

Silver in the vitriolic acid.

100 grs. of pure ſilver require to diſſolve them 530 grs. of real vitriolic acid, whoſe proportion to water is not leſs than that of 1 to $\frac{5}{10}$, and when ſuch a concentrated acid is uſed, it acts ſlightly even in the temperature of 60°; but for a copious

ſolution

ſolution a moderate heat is requiſite. This ſolution affords 30 cubic inches of vitriolic air. Standard ſilver affords more air and requires more acid for its ſolution. The calces of ſilver (that is, ſilver precipitated from its ſolution in nitrous acid by fixed alkalies, and well-waſhed, but which ſtill retains ſome nitrous acid), are ſoluble even in dilute vitriolic acid, without the aſſiſtance of heat. 100 grs. of vitriol of ſilver, formed by precipitation, contain 74 grs. of ſilver, about 17 of real acid, and 9 of water.

Silver in the nitrous acid.

100 grs. of the pureſt ſilver require for their ſolution 36 of mere nitrous acid, diluted with water in the proportion of one part real acid to 6 of water, applying heat only when the ſolution is almoſt ſaturate. If ſpirit of nitre be much more or much leſs dilute, it will not act without the aſſiſtance of heat. The laſt portions of ſilver, thus taken up, afford no air. *Standard* ſilver requires about 38 grs. of real acid to diſſolve the ſame proportion of it. And the ſolution of it affords 20 cubic inches of nitrous air, whereas 100 grs. of ſilver, revived from *luna cornua*, afford about 14.

Silver in the marine acid.

I have not been able to diſſolve ſilver, in its metallic ſtate, in ſpirit of ſalt, yet I believe it may be effected, if ſufficient time be allowed, as Mr. BAYEN, in his Treatiſe on Tin, p. 201. ſays, he diſſolved 3½ grs. of ſilver by digeſting it for ſome days in two ounces of ſtrong ſpirit of ſalt. Leaf ſilver is alſo ſaid to be corroded by ſtrong ſpirit of ſalt, 1 NEWM. 70. The dephlogiſticated marine acid alſo diſſolves it, according to the obſervations of Meſſ. SCHEELE and BERGMAN: and ſo does the phlogiſti-

cated

cated in a vaporous ſtate. 100 grs. of horn ſilver contain 75 of ſilver, nearly 18 of acid, and 7 of water.

Gold in aqua regia.

I made ſeveral experiments with aqua regia, in which the nitrous and marine acids were mixed in different proportions, and found *that* to ſucceed beſt, in which the quantity of real marine acid was to that of the nitrous as 3 to 1, and both as concentrated as poſſible; though if both be very concentrated, it is hard to mix them ſo as to prevent a great quantity from eſcaping, as they effervеſce very violently ſome time after mixture. 100 grs. of gold require 246 grs. real acid for their ſolution, the two acids being in the above mentioned proportion.

The ſpecific gravity of the nitrous acid I uſed was 1,465, and that of the marine 1,178. The ſolution is better promoted by allowing it ſufficient time than by applying heat. The heat I uſed did not exceed 90 or 100°. Very little air is produced, and the ſolution is very ſlow. Aqua regia made with common ſalt or ſal ammoniac and ſpirit of nitre is much leſs aqueous, than that reſulting from an immediate combination of both acids; and hence is the fitteſt for the production of cryſtals of gold.

Gold is alſo ſoluble in the dephlogiſticated marine acid, but in very ſmall quantity, unleſs this acid be in a vaporous ſtate, for in a liquid ſtate it is too aqueous. In vitriolic and nitrous acids it is alſo inſoluble; but the calces of gold are eaſily ſoluble in the marine acid, very ſlightly in the nitrous, and ſcarce at all in the vitriolic. Gold in its metallic ſtate may be diffuſed through, but not diſſolved, by the concentrated nitrous acid.

Mercury in vitriolic acid.

100 grs of quickſilver require for their ſolution 230 grs. of real vitriolic acid, whoſe proportion to that of water is at leaſt as 1 to $\frac{4}{100}$, and alſo a ſtrong heat. The air produced is vitriolic. Precipitate, *per ſe*, is ſtill leſs ſoluble.

100 grs. of vitriol of mercury, produced by precipitation, contain 77 of mercury, 19 of acid, and 4 of water.

Mercury in nitrous acid.

100 grs. of mercury are diſſolved by 28 grs. of real nitrous acid, whoſe proportion to that of water is as 1 to 1 and $\frac{52}{100}$, and without the aſſiſtance of heat. Mercury is alſo ſoluble, but in ſmaller quantity, in a much more dilute acid, with the aſſiſtance of heat. The product of air is about 12 cubic inches or leſs, if heat be not applied. Mr. LAVOISIER found the product of air much greater, which evidently was cauſed by his uſing red or yellow ſpirit of nitre, which already contains much phlogiſton. When I diſſolved a hundred grs. of mercury in three times more acid than was neceſſary for its ſolution, and without heat, I obtained but 7 cubic inches of nitrous air, and the ſolution was green; but, on applying heat when the ſolution was over, I obtained 2 more cubic inches, and then the ſolution was of the colour of oil of olives.

Precipitate, *per ſe*, is much more difficultly diſſolved by nitrous acid than genuine mercury, which I attribute to the attraction of the aërial acid contained in the precipitate.

Mercury in marine acid.

The marine acid, in its common phlogiſticated ſtate, does not act on mercury, at leaſt in its uſual ſtate of concentration;

but Mr. HOMBERG, in the Paris Memoirs for the year 1700, aſſures us, he diſſolved mercury in marine acid, whoſe ſpecific gravity was 1,300, by keeping it ſome months in digeſtion. The authors of the *Cours de Chymie de Dijon* affirm alſo its ſolubility in this acid, though in very ſmall quantity. The dephlogiſticated marine acid, in a vapourous ſtate, certainly acts upon it, though while in a liquid ſtate it is too weak, by reaſon of its dilution.

Precipitate, *per ſe*, is alſo ſoluble in marine acid, with the aſſiſtance of heat. 100 grs. of ſublimate corroſive contain 77 of mercury, 16 of real acid, and 6 of water. 100 grs. of mercurius dulcis contain 86 of mercury, and 14 of acid and water.

Zinc in vitriolic acid.

100 grs. of zinc require for their ſolution 100 grs. of real acid, whoſe proportion to that of water may be as 1 to 8, 10, or 12, applying heat towards the end, when the acid is almoſt ſaturated. A ſmall quantity of black powder always remains undiſſolved. The product of inflammable air is 100 cubic inches. It is ſoluble in the concentrated vitriolic acid, with the aid of heat.

100 grs. of vitriol of zinc contain 20 of zinc, 22 of acid, and 58 of water.

The calces of zinc, if not exceedingly dephlogiſticated, are alſo ſoluble in this acid.

Zinc in nitrous acid.

100 grs. of zinc require for their ſolution 125 grs. of real nitrous acid, whoſe proportion to that of water is as 1 to 12, applying from time to time a ſlight heat. If a concentrated acid

be uſed, leſs will be diſſolved, as much of the acid will eſcape during the efferveſcence. I could procure no nitrous air from the ſolution by any management, as the nitrous acid is in part decompoſed during the operation.

The calces of zinc, if not too much dephlogiſticated, are alſo ſoluble in this acid.

Zinc in Marine Acid.

The ſame quantity of zinc requires of this acid 210 grs. the proportion of real acid in the menſtruum being as 1 to 9, and uſing from time to time a ſlight heat. If a leſs dilute acid be uſed, more real acid will be requiſite, as much of it will eſcape during the efferveſcence.

The calces of zinc are alſo ſoluble in this acid.

Biſmuth in vitriolic acid.

200 grs. of oil of vitriol, whoſe ſpecific gravity was 1,863, diſſolved but three grs. of wiſmuth in a ſtrong heat; but ſlightly dephlogiſticated a greater quantity. 400 grs. of ſpirit of vitriol, whoſe ſpecific gravity was 1,200, diſſolved but one grain. The calces of wiſmuth are much more ſoluble. The ſolution of the 3 grs. afforded 4 cubic inches of vitriolic air.

Biſmuth in nitrous acid:

The ſolution of 100 grs. of biſmuth require but 100 grs. of real nitrous acid, whoſe proportion to water ſhould be as 1 to 8 or 9. In this laſt caſe, a gentle heat may be applied. This ſolution affords 44 cubic inches of nitrous air. The calces of biſmuth are alſo ſoluble in this acid.

Biſmuth in marine acid.

400 grs. of ſpirit of ſalt, whoſe ſpecific gravity is 1,220, diſſolved only 3 or 4 grs. of biſmuth.

Nickel in vitriolic acid.

100 grs. of concentrated vitriolic acid diſſolve about 4 of nickel, with the aſſiſtance of a ſtrong heat. The calces of nickel are much more ſoluble.

Nickel in nitrous acid.

100 grs. of nickel require for their ſolution 112 grs. of nitrous acid, whoſe proportion to water is as 1 to 11 or 12, aſſiſted with a moderate heat. A concentrated acid acts ſo rapidly that much is diſſipated. The product of nitrous air is 79 cubic inches. The calces of nickel are alſo ſoluble in this acid.

Nickel in marine acid.

200 grs. of ſpirit of ſalt, whoſe ſpecific gravity is 1,220, diſſolved 4 or 5 grs. of nickel, without the aſſiſtance of heat. A weaker acid diſſolves leſs, and requires the aſſiſtance of heat. In all theſe caſes of difficult ſolution more of the metal will be taken up by diſtillation and cohobation; but the proportion will be difficult to aſſign.

The calces of nickel are alſo difficultly ſoluble in this acid.

Cobalt in vitriolic acid.

100 grs. of cobalt require 450 grs. of real acid, whoſe proportion to its water is not leſs than 1 to $\frac{1}{4}$, and a heat of

270° at leaſt. By pouring warm water on the dephlogiſticated maſs a ſolution is obtained.

The calces of cobalt are ſtill more ſoluble; even a dilute acid will ſerve.

Cobalt in nitrous acid.

100 grs. of cobalt requires 220 grs. of real nitrous acid, whoſe proportion to water is as 1 to 4, giving towards the end a heat of 180°.

The calces of cobalt are ſoluble in this acid.

Cobalt in marine acid.

100 grs. of ſpirit of ſalt, whoſe ſpecific gravity is 1,178, diſſolves, with the aſſiſtance of heat, 24 grs. of cobalt. A more concentrated acid will diſſolve more.

The calces of cobalt are more ſoluble in this acid.

Regulus of antimony in vitriolic acid.

100 grs. of regulus of antimony require for their ſolution 725 grs. of real acid, whoſe proportion to water is as 1 to $\frac{1}{12}$, and a heat of 400°. More regulus ſhould be employed than is expected to be diſſolved, and the reſulting ſalt requires a large quantity of water to diſſolve it; for the concentrated acid lets fall much when water is added to it. A leſs concentrated acid will alſo diſſolve this ſemi-metal, but in ſmaller quantity.

The calces of antimony, even diaphoretic antimony, are ſomething more ſoluble.

Regulus of antimony in nitrous acid.

100 grs. of this ſemi-metal require 900 grs. of real nitrous acid, whoſe proportion to water is as 1 to 12, aided with a heat

heat of 110°. The ſolution, however, becomes turbid in a few days.

The calces of antimony are ſoluble in a much leſs degree.

Regulus of antimony in marine acid.

100 grs. of ſpirit of ſalt, whoſe ſpecific gravity is 1,220, diſſolve about 1 gr. of regulus, with the aſſiſtance of a ſlight heat. Spirit of ſalt, whoſe ſpecific gravity is 1,178, alſo acts upon it, but diſſolves ſtill leſs. I believe the concentrated acid would, in a long time, and with the help of a gentle heat, diſſolve much more of it.

The calces of antimony are much more ſoluble in this acid.

Regulus of arſenic in vitriolic acid.

200 grs. of oil of vitriol, whoſe ſpecific gravity is 1,871, diſſolve 18 of regulus of arſenic in a heat of 250°. Of theſe about 7 cryſtalize on cooling, and are ſoluble in a large quantity of water.

The calces of arſenic are more ſoluble in this acid.

Regulus of arſenic in nitrous acid.

100 grs. of this ſemi-metal require 140 grs. of real nitrous acid, whoſe proportion to water is as 1 to 11, and the aſſiſtance of heat. It is ſoluble in a leſs or more concentrated acid, but in a leſſer degree. This ſolution affords 102 cubic inches of nitrous air. The barometer at 30, and the thermometer at 60.

The calces of arſenic are alſo ſoluble in this acid.

Regulus of arſenic in marine acid.

100 grs. of ſpirit of ſalt, whoſe ſpecific gravity is 1,220, diſſolve 14 grs. of regulus of arſenic; the marine acid, in its common

common dilute ſtate, that is, whoſe ſpecific gravity is under 1,17, does not at all affect it.

The calces of arſenic are leſs ſoluble in this acid than in the vitriolic or nitrous.

We have now gone through moſt of the baſes to which acids are capable of uniting (manganeſe and platina I have purpoſely omitted, as I was not poſſeſſed of a ſufficient quantity of either in that degree of purity requiſite for exact experiments). We have alſo ſeen the quantity of the mineral acids requiſite to ſaturate each baſis, except the metallic baſes, all of which require an exceſs of acid, not only to diſſolve them, as in moſt caſes much of it flies off with the phlogiſton in an aërial form, but alſo to keep them in ſolution. The quantity of any baſis, taken up by a given quantity of any of theſe acids, is eaſily found; for if 100 grs. of any baſis take up, at the point of ſaturation, or require for their ſolution, the quantity *a* of any acid, the quantity taken up or diſſolved by 100 grs. of that acid will be $\frac{10000}{a}$.

The proportion of ingredients which I have aſſigned to different neutral ſalts appears, at firſt ſight, very different from that which Mr. BERGMAN has aſcribed to them. This for ſome time made me very uneaſy, as I have the higheſt confidence in the ſkill and judgement of that excellent chymiſt; but on a ſtrict examination I have found, that the difference is more apparent than real. Mr. BERGMAN has never attempted to aſcertain the quantity of *real* acid in any ſubſtance; but has, according to the cuſtom of all preceding writers, beſtowed the title of *acid* on thoſe liquids which contain it in the moſt concentrated, or at leaſt in a very concentrated ſtate, but which

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ſtill confeſſedly contain ſome undetermined proportion of water, and by the quantity of water he commonly underſtands that which is retained by cryſtallization: thus, in his firſt vol. p. 137. he ſays, that 100 grs. of vitriol of iron contain 23 of iron, 39 of vitriolic acid, and 38 of water. But in his treatiſe *De Productis Vulcanicis*, § 12. he ſays, that 100 grs. of vitriol of iron contain 24 of iron, 24 of *dephlegmated* vitriolic acid, and 52 of water; and this laſt calculation ſcarcely differs from mine, as I aſſign to 100 grs. of vitriol 25 of iron, 20 of real vitriolic acid, and 55 of water. The difference manifeſtly ariſes from the quantity of water ſtill contained in his dephlegmated acid. The moſt material difference between us regards the quantity of the mineral acids taken up by alkalies; for, according to his and Mr. SCHEFFER's experiments, they take up more of the vitriolic than of the nitrous, and more of the nitrous than of the marine; whereas, according to Mr. HOMBERG's, Dr. PLUMMER's, Mr. WENZEL's, and my experiments, this does not happen. This difference ariſes in all probability from the different degrees of evaporation by which the cryſtals of theſe ſalts are obtained; for which reaſon I did not examine the quantity of the cryſtals, which muſt be variable, but that of dry ſalt, left after thorough evaporation. With regard to the quantity of earth and metallic baſis in different ſalts, Mr. BERGMAN's experiments and mine agree almoſt intirely.

The advantages reſulting from theſe inquiries are very conſiderable, not only in promoting chymical ſcience, which, being a phyſical analyſis of bodies, eſſentially requires an exact determination, as well of the quantity and proportion, as of the quality of the conſtituent parts of bodies, but alſo in the practical way. Thus, in the firſt place, it is well known, that

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ſeveral important proceſſes are very inaccurately deſcribed by antient chymical writers, and even by ſome of a modern date, they frequently, for inſtance, deſcribe the acid they employed by reference to the quantity of fixed alkali, earth, or metal, a given quantity of ſuch acid was capable of neutralizing or diſſolving. Now the foregoing obſervations immediately inform us of the quantity of real acid capable of performing that effect; the remainder, therefore, muſt have been water; and the quantity of real acid and water being known, the ſpecific gravity is eaſily found by the help of the foregoing tables, and thus an acid of the ſame ſtrength may be formed. Thus SCHLUTTER, in the beſt treatiſe on Eſſaying yet extant *, informs us, that the beſt aqua fortis for parting ſilver from gold is that of which a pound diſſolves one mark, that is, half a pound of ſilver: then 1000 grs. of it ſhould diſſolve 500 of ſilver. Now, by the foregoing experiments, we find, that 100 grs. of alloyed ſilver require 38 of real acid for their ſolution; conſequently 500 grs. of ſilver will require 190 grs.; conſequently 1000 grs. of ſuch ſpirit of nitre ſhould contain 190 grs. of real acid and 810 of water. Then recurring to the table of the nitrous acid, I find, by the rule of proportion, that the ſpecific gravity of this acid muſt be about 1,261; for as 190 is to 810, ſo is 393 acid to 1675 of water. This proportion of water is ſomewhat greater than that I uſed, but SCHLUTTER uſes a ſand heat.

2dly, The importance of this knowledge in the art of pharmacy is very obvious, eſpecially with regard to medicines formed of metallic ſubſtances, whoſe powers depend on the proportion of their ingredients, and their action on each other.

3dly, This degree of preciſion muſt tend conſiderably to the

* Vol. I. p. 332. French edition.

improvement of the arts of dying and enamelling, the proceſſes by which many of their ingredients are procured being at preſent much too vague. Thus the proceſs at preſent uſed for preparing the precipitate of CASSIUS frequently fails, the ſtrength of the acids not being ſufficiently aſcertained.

4thly, The uſes of this knowledge in the examination of mineral waters, and in eſſaying of ores, have been amply proved in the elaborate treatiſes which the celebrated BERGMAN has lately given us on theſe ſubjects. And I may further add, that the knowledge of the quantity of acid requiſite for the ſolution of different metallic ſubſtances may alſo furniſh us with a new criterion for diſtinguiſhing them from each other, and the purer from their alloys, and in ſome caſes inform us of the quantity and quality of the alloy: thus, 100 parts pure ſilver require leſs of the nitrous acid to diſſolve them than 100 grs. of ſtandard ſilver; thus alſo, by diſſolving in ſpirit of ſalt any metallic ſubſtance ſufficiently ſoluble therein, we may know whether it contains the ſmalleſt particle of ſilver, quick-ſilver, or arſenic, as theſe are almoſt inſoluble therein, or of regulus of antimony, cobalt, nickel, or wiſmuth, of which it alſo takes but a ſmall proportion.

But the end which of late I had principally in view, was to aſcertain and meaſure the degrees of affinity or attraction that ſubſiſt betwixt the mineral acids, and the various baſes with which they may be combined, a ſubject of the greateſt importance, as it is upon this foundation that chymiſtry, conſidered as a ſcience, muſt finally reſt; and though much has been already done, and many general obſervations laid down on this head, yet ſo many exceptions have occurred even to ſuch of theſe obſervations as ſeemed to have been moſt firmly eſtabliſhed, that not only a variety of tables of affinity have been formed,

but many very eminent chymiſts have been induced to doubt, whether any general law whatſoever could be traced. But, as the judicious BERGMAN well obſerves, it were much more reaſonable to examine the circumſtances of theſe exceptions, which undoubtedly ariſe from the introduction of new powers, and lay down rules qualified with ſuch reſtrictions as are obſerved in the action of theſe antagoniſt powers. This is the plan I have followed; but before I proceed to explain myſelf, I muſt open the ſubject in a more general way.

Chymical affinity or attraction is that power by which the inviſible particles of different bodies intermix and unite with each other ſo intimately as to be inſeparable by mere mechanical means. In this reſpect it differs from magnetic and electrical attraction. It alſo differs from attraction of coheſion in this, that the latter takes place betwixt particles of almoſt all ſorts of bodies whoſe ſurfaces are brought into immediate contact with each other; for chymical attraction does not act with that degree of indifference, but cauſes a body already united to another to quit that other and unite with a third, and hence it is called *elective* attraction. Hence attraction of coheſion often takes place betwixt bodies that have no chymical attraction to each other; thus regulus of cobalt and wiſmuth have no chymical attraction to each other, for they will not unite in fuſion, yet they cohere with each other ſo ſtrongly, that they can be ſeparated only by a ſtroke of a hammer.

Hence bodies, which refuſe to unite to each other chymically when they are moſt minutely divided, as when both are in a vaporous or aërial ſtate, or when both are in a liquid ſtate, may be judged, in the firſt caſe, to have none; or in the ſecond caſe, to have at beſt but a very ſmall affinity to each other. But thoſe that unite, when one of them only is in a liquid ſtate,

may be ſaid to have a ſtrong affinity to each other, and it is thus that acids unite to alkalies, earths, and metals, for the moſt part.

In order to determine the degrees of affinity, Mr. GEOFFROY has laid it down as a general rule, that when two ſubſtances are united, and either quits the other to unite to a third, that which thus unites to this third ſubſtance muſt be ſaid to have a greater affinity to it than to the ſubſtance it has quitted. This undoubtedly is the caſe when only two attractive powers are concerned; thus, when ſelenite is decompoſed by a cauſtic fixed alkali, it is evident, that the vitriolic acid has a ſtronger affinity to the alkali than to the earth; but in many caſes a decompoſition, ſeemingly ſingle, is in fact double, and the reſult of the action of more than two powers, and then it is not eaſy to know which is the greateſt, nor conſequently to determine the degree of attraction; for inſtance, the vitriolic acid unites to a mild fixed alkali, and expels the fixed air from it, yet it does not neceſſarily follow, that the vitriolic acid attracts, or is attracted, by the alkali more ſtrongly than the aërial acid; for though there appears here only a ſingle decompoſition, yet in reality a ſort of double decompoſition takes place, the vitriolic acid giving out its fire to the aërial, while the aërial reſigns the alkali to the vitriolic; and hence a decompoſition might well take place, even on the ſuppoſition that the affinity of both acids to the alkali was equal: therefore, to attain any certainty in this matter, it is neceſſary to aſcertain the quantity and force of each of the attractive powers, and denote it by numbers.

Mr. MORVEAU was the firſt who perceived the neceſſity of this calculation, and he has accordingly communicated to us, in numbers, a table of the attractive power of mercury with reſpect to metals; but his method is incapable of being generalized.

ſized. Mr. WENZEL had alſo an eye to ſuch calculation; but his method is much more defective. It is only this, "To "diſcover (ſays he) the quantity of affinity which the nitrous "acid bears to the different ſubſtances with which it is capable "of uniting, let ſmall equal cylinders of each of the metals "be covered over, except at one end, with melted amber, and "then expoſed to equal quantities of the ſame ſpirit of nitre, "and in the ſame temperature; then let the times of the ſolu- "tion of each be noted. *The affinity of the acid to each of theſe "metals will be inverſely as the times neceſſary for the ſolution of "equal quantities of them.*" And as he well knew that ſpirit of nitre, of the ſame degree of concentration, would not act equally on each of them, he required that it ſhould be diluted in ſome caſes, and undiluted in others, and allowance to be made for this in the ſubſequent calculation. But alkalies and earths are here intirely omitted; and even as to metals no concluſion can be drawn by this method. Tin and regulus of antimony are moſt rapidly attacked by this acid, lead and copper much more ſlowly; yet it is well known, that its affinity to lead is much ſtronger than its affinity to tin, and its affinity to copper greater than to regulus of antimony. Silver and quickſilver are more ſlowly diſſolved, and yet the affinity of the nitrous acid to theſe metals, as will be ſeen in the ſequel, is by far the greateſt.

Neither can this method be in any wiſe applied to the eſtimation of the affinities of the other mineral acids; for though the vitriolic and marine acids diſſolve very ſlowly, difficultly, and ſparingly, ſeveral metals that are copiouſly and readily diſſolved by the nitrous, yet they both have a ſtronger affinity to thoſe very metals than the nitrous has to them, as is evident with regard to ſilver, mercury, and lead, which are precipitated from the

nitrous

nitrous by the vitriolic and marine, though the two firſt are inſoluble in the marine, and all three difficultly ſoluble in the vitriolic. Accordingly, we do not find that Mr. WENZEL has ever made the propoſed experiments, at leaſt he makes no mention of their reſult.

The diſcovery of the quantity of real acid in each of the mineral acid liquors, and the proportion of real acid, taken up by a given quantity of each baſis at the point of ſaturation, led me unexpectedly to what ſeems to me the true method of inveſtigating the quantity of attraction which each acid bears to the ſeveral baſes to which it is capable of uniting; for it was impoſſible not to perceive,

Firſt, That *the quantity of real acid, neceſſary to ſaturate a given weight of each baſis, is inverſely as the affinity of each baſis to ſuch acid.*

Secondly, That *the quantity of each baſis, requiſite to ſaturate a given quantity of each acid, is directly as the affinity of ſuch acid to each baſis.*

Thus 100 grs. of each of the acids require for their ſaturation a greater quantity of fixed alkali than of calcareous earths, more of this earth than of volatile alkali, more of this alkali than of magneſia, and more of magneſia than of earth of allum, as may be ſeen in the following table.

Quantity of baſis taken up by 100 *grs. of each of the mineral acids.*

	Veg. fixed alkali. Grs.	Min. alkali. Grs.	Calcar. earth. Grs.	Vol. alkali. Grs.	Mag-neſia. Grs.	Earth of allum. Grs.
Vitriolic acid	215	165	110	90	80	75
Nitrous acid	215	165	96	87	75	65
Marine acid	215	158	89	79	71	55

As

As theſe numbers agree with what common experience teaches us concerning the affinity of theſe acids with their reſpective baſes, they may be conſidered as adequate expreſſions of the quantity of that affinity, and I ſhall in future uſe them as ſuch. Thus the affinity of the vitriolic acid to fixed vegetable alkali, that is, the force with which they unite, or tend to unite, to each other, is to the affinity with which that ſame acid unites to calcareous earth as 215 grs. to 110; and to that which the nitrous acid bears to calcareous earth as 215 grs. to 96, &c. But before I proceed further in the compariſon of theſe forces, it is neceſſary to ſay ſomething of the nature of *ſaturation*.

A body is ſaid to be *ſaturated* with another, when it is ſo intimately combined with that other as to loſe ſome peculiar characteriſtic property, which it poſſeſſes when free from that other. Thus acids poſſeſs the property of changing the juice of turnſol, or infuſion of litmus, *red*. According to Mr. BERGMAN one grain of the moſt concentrated oil of vitriol will give a viſible redneſs to 172,300 grs. of this infuſion, and one cubic inch of water, ſaturated with fixed air (the weakeſt of all acids, as is generally thought) of which water takes up only about its own bulk, and conſequently 253 grs. take up only about half a grain, reddens 50 cubic inches, that is, about 12,650 grs. of the infuſion. When acids loſe this property they are ſaid to be ſaturated: and if both bodies are ſaturated, the compound is ſaid to be *neutralized*.

If an acid be united to leſs of any baſis than is requiſite for its ſaturation, its affinity to the deficient part of its baſis is as the ratio which that deficient part bears to the whole of what the acid can ſaturate. Thus if 100 grs. of vitriolic acid be united to 55 parts only of calcareous earth, its affinity to the deficient

55 parts

55 parts ſhould be eſtimated half of its whole affinity, as 55 is the half of 110; but its affinity to the retained part is as its whole affinity.

I ſhall now ſhew how all decompoſitions, in which theſe three acids and the above mentioned baſes are alone concerned, may eaſily be explained.

In all decompoſitions we muſt conſider, firſt, the powers which reſiſt any decompoſition, and tend to keep the bodies in their preſent ſtate; and, ſecondly, the powers which tend to effect a decompoſition and a new union. The firſt I ſhall call *quieſcent* affinities, and the ſecond ſort *divellent*.

A decompoſition will always take place when the ſum of the divellent affinities is greater than that of the *quieſcent*; and, on the contrary, no decompoſition will happen when the ſum of the *quieſcent* affinities is ſuperior to, or equal to, that of the *divellent*: all we have to do, therefore, is to compare the ſums of each of theſe powers. Thus, if the ſolutions of tartar vitriolate and nitrous ſelenite be mixed, a double decompoſition will take place, a true ſelenite and nitre being the reſult of ſuch mixture.

Quieſcent affinities.		Divellent affinities.	
Vitriolic acid to fixed veget. alkali	215	Vitriolic acid to calcareous earth	110
Nitrous acid to calcareous earth	96	Nitrous Acid to vegetable alkali	215
Sum of the quieſcent affinities	311	Sum of the divellent —	325

Hence a double decompoſition muſt neceſſarily happen.

The ſame double decompoſition will be produced if, inſtead of tartar vitriolate, GLAUBER's ſalt be uſed, 1 MARGR. 392.; for the ſum of the quieſcent affinities is 261, and that of the divellent 275. So alſo, if *vitriolic ammon.* be uſed for the ſum

of

of the quieſcent is 186, and that of the divellent 195, or Epſom ſalt, 1 MARGR. 390. Mem. Par. 1778, p. 339. or *allum*, 1 MARGR. 387. The determinations, however, with regard to allum are not quite ſo exact as the foregoing; becauſe allum, whether vitriolic, nitrous, or marine, conſtantly retains an exceſs of acid, the exact point of ſaturation cannot be found as I have already remarked, and is well known: however, the ſuperiority is on the ſide of the divellent affiuities, as it ſhould be. If, inſtead of a ſolution of nitrous ſelenite, that of marine ſelenite be mixed with the ſolutions of the above mentioned vitriolico-neutral ſalts, the ſame ſort of double decompoſition will happen, and a true ſelenite will be formed, 1 MARGR. 382; and on calculation it will be conſtantly found, that the ſum of the divellent conſtantly exceeds that of the quieſcent affinities.

So alſo, if a ſolution of tartar vitriolate be mixed with a ſolution of nitrous or marine Epſom, a double decompoſition will take place, though no viſible change will appear in the mixed liquor, as vitriolic Epſom is exceeding ſoluble in water, and therefore is not precipitated as ſelenite is, on account of its inſolubility, Mem. Par. 1778, p. 338. In the firſt caſe, the ſum of the quieſcent powers is 290, and of the divellent 295; in the ſecond caſe, that of the quieſcent is 286, and of the divellent 295.

If a ſolution of GLAUBER's ſalt be mixed with that of nitrous or marine Epſom, an inviſible double decompoſition will alſo happen, Mem. Par. ibid. Hence Mr. QUATREMERE DIJON-VAL, who lately denied this double decompoſition (ROZ. Mai 1782, p. 392.) was certainly deceived. In the firſt caſe, the ſum of the quieſcent affinities is 240, and of the divellent 245; in the ſecond caſe, that of the quieſcent is 236, and of

the divellent 238. Further, if a ſolution of nitre be mixed with a ſolution of marine ſelenite, an inviſible double decompoſition will enſue, Mem. Par. 1778, p. 341.; the ſum of the quieſcent powers being 304, and of the divellent 313.

If a ſolution of nitrous Epſom be mixed with that of marine ſelenite, a double decompoſition will be the conſequence, 17 ROZ. 393.: the ſum of the quieſcent affinities being 164, and that of the divellent 167.

From all which I collect, firſt, that the quantity of each affinity, as here determined, perfectly coinciding with all the facts hitherto known, which are pretty numerous, may be looked upon as exact or nearly ſo. 2dly, That theſe decompoſitions are perfectly conſiſtent with the ſuperior affinity which hitherto has been generally aſcribed to the vitriolic and nitrous acids with fixed alkalies over that which theſe acids bear to earths, and do not in the leaſt infringe the received laws of affinities, as Meſſ. MARHER, MONNET, and lately Mr. CORNETTE, in the Memoirs of Paris for 1778, p. 339. do inſinuate.

There is a fact, however, in that valuable repoſitory of chymical knowledge, Mr. CRELL's Chymical Journal *, which at firſt ſight ſeems contrary to one of the above determinations; it is there ſaid, that if ſolutions of one part allum and two parts common ſalt be mixed together, evaporated to a certain degree, and ſet to cryſtallize, a GLAUBER's ſalt will be found; yet in this caſe the ſum of the quieſcent affinities is 233, and that of the divellent but 223. I repeated this experiment without ſucceſs, and indeed the author owns it never ſucceeds but during the moſt intenſe cold.

If it does ſucceed at all, the decompoſition muſt ariſe from a large exceſs of acid in the allum, which acted upon and de-

* 6 THEIL. p. 78.

compoſed

composed the common salt; and this explanation is confirmed by the small proportion of GLAUBER's salt, which is said to be obtained by this process; for from 30 lbs. of common salt and 16 lbs. of allum only, 15 lbs. of GLAUBER's salt were produced; whereas, if the whole of the allum were decomposed, there should be formed, according to my computation of the proportion of acid in different salts, 29½ lbs., and, according to Mr. BERGMAN's, 22 lbs. of GLAUBER's salt.

Besides these powers there exists another which neutral salts possess, of uniting to certain substances, without suffering any, or but a very small, decomposition; and thus forming *triple* salts, and sometimes quadruple. This often causes anomalies, and has not as yet been sufficiently investigated *. Volatile alkalies in particular possess this power; and hence, perhaps, arises the difference between Mr. BERGMAN's table and mine, with regard to them and magnesia; for though, when perfectly caustic, they do not perfectly precipitate magnesia from Epsom salt, it is because they combine with this salt and form a triple salt.

According to my table, the three mineral acids have the same affinity to vegetable fixed alkalies, which will undoubtedly appear extraordinary to many, as it is well known, that the vitriolic acid decomposes both nitre and salt of sylvius; but it should be remarked, that tartar vitriolate is also decomposed by the nitrous and marine acids, as Mr. BAUMÉ, MARGRAAF, and BERGMAN, have found; and nitre is decomposed by the marine acid, as Mr. CORNETTE has shewn at large in the Memoirs of Paris for 1778; and not only these salts, but also GLAUBER's salt and vitriolic ammoniac, are decomposed by the

* In my next paper I shall examine some exceptions arising from this source.

nitrous acid; and alſo theſe ſalts, together with cubic nitre and nitrous ammoniac, are decompoſed by the marine acid, as Mr. BERGMAN and Mr. CORNETTE have remarked: all which ſhew, that theſe decompoſitions are the effect of a double affinity, or at leaſt of compound forces. I always ſuſpected they aroſe from the different capacities of theſe acids for elementary fire; but as the ſubject appeared to me of importance, for greater certainty I made a ſeries of experiments which differ from thoſe hitherto made in ſeveral reſpects, particularly in this, that no heat was applied, and the decompoſitions were diſcovered, not by cryſtallization, but by teſts.

Firſt, I procured equal weights of each of the mineral acids, containing each the ſame quantity of real acid; and throwing each ſuddenly on an ounce of the ſame oil of tartar, I had the following reſults, the temperature of all, before mixture, being 68° of FAHRENHEIT. 100 grs. of vitriolic acid, containing 26,6 grs. of real acid projected on 480 of oil of tartar, raiſed the thermometer to 138°.

100 grs. of ſpirit of nitre, which alſo contained 26,6 grs. real acid, projected in the ſame manner on 480 grs. of the ſame oil of tartar, produced a heat of 120°.

100 grs. of ſpirit of ſalt, whoſe ſpecific gravity was 1,220, and which contained 26,6 grs. of real acid, projected on the above quantity of the ſame oil of tartar, raiſed the thermometer from 69 to 129°.

Hence it follows, that the vitriolic acid contains more ſpecific fire, or at leaſt gives out more on uniting to fixed alkalies, than either the nitrous or marine; and, therefore, when the vitriolic acid comes in contact with either nitre or ſalt of ſylvius, its fire paſſes into theſe acids, which are thereby rarefied to a great degree, and are thus expelled from their alkaline baſis,

baſis, which is then ſeized on by the vitriolic. This explanation is confirmed by the following experiments.

Into 400 grs. of ſpirit of vitriol, whoſe ſpecific gravity was 1,362, I put 60 grs. of nitre. The thermometer fell from 68 to 60, and during this time the nitrous acid was not expelled, for I put in ſome filings of copper, and they were not in the leaſt acted upon; but in five minutes after, they viſibly effervesced, which ſhews that the nitrous acid began to be expelled.

Again, to 400 grs of oil of vitriol, whoſe ſpecific gravity was 1,870, I put 60 grs. of nitre: the thermometer immediately roſe from 68 to 105°, and the nitrous acid was expelled in the form of a viſible fume. Theſe experiments prove, firſt, that neutral ſalts are not decompoſed, by mere ſolution, in an acid different from that which they poſſeſs. 2dly, That the nitrous acid, being converted into vapour, had imbibed a large quantity of fire. But as the vitriolic, in both theſe experiments, was in much larger quantity than was neceſſary to ſaturate the alkaline baſis of the nitre, I put 60 grs. of nitre into 64 of the above ſpirit of vitriol, which contained the ſame quantity of real vitriolic acid as the 60 grs. of nitre did of the nitrous, and added 40 grs. of water, and alſo a few grains of filings of copper. In leſs than two hours the copper was acted upon, and conſequently the nitrous acid was expelled.

Again to about 400 grs. of oil of vitriol, whoſe ſpecific gravity was 1,870, I put 100 grs. of common ſalt; it immediately effervesced, and gave out the marine acid in the form of a white vapour. A thermometer held in the liquor roſe but 4°; but when placed in the froth it roſe to 10°, and fell again on being put into the liquor: whence it follows, that the vitriolic acid gave out its fire to the marine, and that this latter received

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more than it could abſorb even in the ſtate of vapour, and hence communicated heat to the contiguous liquor.

From theſe experiments it is evident, that the nitrous and marine acids receive fire from the vitriolic, and are thrown into a vapourous ſtate, or at leaſt ſo much rarefied as to be expelled from their alkaline baſis, notwithſtanding that their affinity to that baſis may be equally ſtrong with that of the vitriolic.

I next proceeded to examine how tartar vitriolate and GLAUBER's ſalt are decompoſed by the nitrous acid. Into 400 grs. of ſpirit of nitre, whoſe ſpecific gravity was 1,355, and which contained about 105 grs. of real acid, I put 60 of pulveriſed tartar vitriolate. The thermometer, which ſtood at 68°, was not in the leaſt affected by ſtanding in this mixture, and there was ſcarce any ſign of ſolution. To try whether the vitriolic acid was diſengaged, I threw into the liquor a few grains of powdered regulus of antimony: in 24 hours the vitriolic acid was in part diſengaged, for the regulus was acted upon, and the liquor became greeniſh. This ſemi-metal being ſoluble in a mixture of the vitriolic and nitrous acids, but in neither ſingly, however, a great part of the tartar vitriolate ſtill remained undiſſolved. Afterwards I put the ſame quantity of tartar vitriolate into 400 grs. of ſpirit of nitre, whoſe ſpecific gravity was 1,478; the thermometer roſe from 67 to 79°, the tartar vitriolate was quickly diſſolved, and the regulus of antimony ſhewed the vitriolic acid was diſengaged.

Hence it follows, that in the laſt experiment the nitrous acid having the ſame affinity to the alkaline baſis as the vitriolic, but giving out, during the ſolution, more fire than was neceſſary to perform the ſolution, the vitriolic receiving this fire was diſengaged; for as it cannot unite to alkalies without giving out fire, ſo when it receives back that fire it muſt quit them. The

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reaſon why the nitrous acid, which ſpecifically contains leſs fire than the vitriolic, gives out ſo much, is, that its quantity in both theſe experiments is far greater than that of the vitriolic, it being in the firſt as 105 to 17; and in the ſecond as 158 to 17.

For this reaſon, to 60 grs. of ſpirit of nitre, whoſe ſpecific gravity was 1,355, I added 1000 grs. of water, and into this dilute acid I put 60 grs. of tartar vitriolate, which contained exactly the ſame quantity of acid as the 60 grs. of ſpirit of nitre. After eight days the tartar vitriolate was almoſt intirely diſſolved, yet I could perceive no ſign of its decompoſition, and after evaporation no nitre was found. Hence I conclude, that the nitrous acid can never decompoſe tartar vitriolate without the aſſiſtance of heat, but when its quantity is ſo great that it contains conſiderably more fire, and by the act of ſolution is determined to give out this fire. The decompoſition of GLAUBER's ſalt and vitriolic ammon. (neither of which, as Mr. BERGMAN obſerved, is ever total) may be explained in the ſame manner; whereas the vitriolic, ever ſo dilute, decompoſes both nitre and nitrous ammoniac totally. Tartar vitriolate is alſo decompoſed by the marine acid, though very ſlowly, for the ſame reaſon, and in the ſame circumſtances, as it is decompoſed by the nitrous acid, as appears by the following experiments. Into 400 grs. of ſpirit of ſalt, whoſe ſpecific gravity was 1,220, I put 60 grs. of tartar vitriolate. The thermometer was not in the leaſt affected, and the ſalt diſſolved very ſlowly. To try whether the vitriolic acid was diſengaged, I added ſome pulveriſed biſmuth; in twelve hours part of the biſmuth was diſſolved, and could not be precipitated by the affuſion of water, a ſign that it was held in ſolution by the compound acid, which alone hath the property of

preventing its precipitation by the affuſion of water, as Mr. WENZEL has diſcovered. Here the quantity of marine acid was much greater than that of the vitriolic, and conſequently it contained more fire; but this circumſtance alone is not ſufficient, it muſt beſides be determined to give out that fire by the act of ſolution. This appears by the experiments of Mr. CORNETTE; for when he mixed half an ounce of tartar vitriolate, *previouſly diſſolved* in water with two ounces of ſpirit of ſalt, the tartar vitriolate was not decompoſed, Memoirs, Paris, 1778, p. 49.; for it being already diſſolved, no cold or heat was generated by mixing it with the ſpirit of ſalt, and conſequently the latter did not give out any fire. Mr. CORNETTE alſo obſerved, that GLAUBER's ſalt is eaſier decompoſed by the marine acid than tartar vitriolate; this I have alſo experienced, and the reaſon is, firſt, becauſe GLAUBER's ſalt is more eaſily ſoluble in ſpirit of ſalt than tartar vitriolate; and, ſecondly, becauſe its alkaline baſis takes up a greater quantity of the real marine acid than of the vitriolic, whereas the baſis of tartar vitriolate takes up an equal quantity of both acids; conſequently the marine gives out more fire in uniting to the baſis of GLAUBER's ſalt than on uniting to that of tartar vitriolate.

Vitriolic ammoniac is alſo decompoſed by the marine acid for the ſame reaſon; but in all theſe caſes the quantity of the marine acid muſt much exceed that of the vitriolic, or no decompoſition will take place. The decompoſition of nitro-neutral ſalts by the marine acid depends on the ſame principles. Mr. CORNETTE found, that cubic nitre was more eaſily decompoſed than priſmatic nitre, and accordingly, during the ſolution of priſmatic nitre, only 3° of cold were produced; but, during that of cubic nitre, the thermometer fell 6°, a ſign that the ſpirit of ſalt gave out more fire in the latter caſe than

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in the former, and its quantity must always be greater than that of the nitrous acid contained in the mineral alkaline basis, because this basis requires for its saturation more of the marine than of the nitrous acid, as we have already seen.

Yet the nitrous acid, in its turn, decomposes salt of sylvius and common salt, as Mr. MARGRAF has shewn; but it must always be in greater quantity than the marine, in order to contain a sufficient quantity of fire for that effect. To 400 grs. of colourless spirit of nitre, whose specific gravity was 1,478, I put 60 grs. of common salt, it quickly effervesced and grew red; yet the thermometer rose but 2°, a sign that the marine acid had absorbed the greater part of the fire which the nitrous had given out, and was thus expelled: besides, in this case, the superior affinity of the nitrous acid to the mineral basis hastened the decomposition; and hence the decomposition happens without solution, whereas the marine acid does not decompose cubic nitre until it has dissolved it, which is worthy of notice. This mutual expulsion of the nitrous and marine acids by each other is the true reason why aqua regia may be made, as well by adding nitre or nitrous ammoniac to spirit of salt, as by adding common salt or sal ammoniac to spirit of nitre, as Mr. CORNETTE has well remarked.

Selenite is decomposed neither by the nitrous nor by the marine acids, as Messl. CHAPTAL and CORNETTE have observed. The reason is evident on the above principles; it is dissolved by neither without the assistance of heat, and then the solution is performed by a *foreign* heat, and not by that which these acids give out when they act without the assistance of heat.

Lastly, whenever a vitriolico-neutral salt, decomposed by either the nitrous or marine acid, is evaporated to a certain degree, the vitriolic expels these acids in its turn; for the free

part of the former acids being expelled by the heat of evaporation, the neutral ſalts begin to cryſtallize, and conſequently give out heat; but the vitriolic being then in greater proportion re-acts on theſe ſalts, reſtores their ſpecific fire to their acid principle, and recombines with their alkaline baſis, as already explained.

Hence, though allum is in reality decompoſed by the nitrous and marine acids, yet when the ſolution of it in either of theſe acids is evaporated to a certain degree, the vitriolic acid, of which it contains a larger proportion than any other terrene ſalt, re-acts on the nitrous and marine allums, and expels their acids, as Mr. CHAPTAL has ſhewn.

In explaining theſe phenomena I have all along ſuppoſed the doctrine of Dr. BLACK to be well known, *viz.* that ſolids abſorb heat during their ſolution. Both the heat and cold, produced in different ſolutions, ſeem to me to depend on the ſame principle. If the menſtruum gives out only *ſo much* of its fire as the ſolvend can abſorb, or *leſs*, then cold is produced; but if it gives out *more* of its ſpecific fire than the ſolvend can abſorb, this ſurplus becomes ſenſible, and affects the thermometer by producing heat in proportion to its quantity.

Of the affinity of the mineral acids to metallic ſubſtances.

Having thus, in every inſtance, eſtabliſhed the agreement betwixt the quantity of any alkaline or terrene baſis, taken up at the point of ſaturation by a given weight of any of the three mineral acids, and the quantity of affinity which each of theſe acids bears to ſuch baſis, I naturally extended my views to metallic ſubſtances, to try whether this coincidence could be traced with regard to them alſo; but the difficulties that occurred in this inquiry were ſo great, that the ſame degree of certainty muſt not be expected as in the foregoing part.

Metallic

Metallic ſubſtances, when freeſt from all foreign mixture, are obtained either in a reguline ſtate, or in that of a calx. Theſe calces, if formed by fire, are conſtantly combined with more or leſs of the aërial acid, which is very difficultly extracted from them, and very ſoon re-abſorbed; and if formed by ſolution, they as conſtantly retain a portion of their ſolvent or precipitant, ſo that the preciſe weight of the really metallic part is difficultly aſcertained. But though this ſhould eaſily be effected, ſtill they would for the moſt part be unfit for my purpoſe; becauſe moſt of them, when much dephlogiſticated, are inſoluble in ſome or all the acids: hence I choſe metals in their metallic ſtate for the ſubject of my experiments. Theſe conſiſt of ſpecifically different earths and phlogiſton, and of this they muſt loſe a part before they can be diſſolved in acids; but, beſides that which eſcapes in an aërial form, much more of it, though ſeparated from the metallic earth, is yet retained in the ſolution by the compound of acid and calx. It is this calx, thus differently dephlogiſticated by the different acids, whoſe proportion I endeavoured to aſcertain.

The great difficulty that occurred in this inquiry was, that of finding the exact quantity of acid neceſſary to ſaturate the metallic ſubſtances; for all metallic ſolutions turn ſolution of litmus red, and conſequently contain an exceſs of acid. And the reaſon is, becauſe the ſalts, formed by a due proportion of metallic calx and acid, are nearly inſoluble in liquids that do not contain a further quantity of acid; and in ſome caſes this quantity, and even its *proportion to the aqueous part of the liquor*, muſt be very conſiderable, as in ſolutions of biſmuth. Hence I in vain endeavoured, by cauſtic alkalies and lime-water, to deprive theſe ſolutions of this exceſs; for when deprived even of only part of it, many of the metals precipitated, and all

would, if deprived of the whole of it. On this account I was obliged to uſe different methods, of which I ſhall here give an inſtance. With regard to the ſolution of ſilver in the nitrous acid, as it could be had extremely ſaturate I began with it. 657 grs. of this ſolution contained, according to my calculation, and allowing for the quantity of acid carried off in the nitrous air, 31,3^{3} grs. of real acid, and 100 grs. of ſilver. Of this ſolution I found that 9 grs. gave a viſible *red tint* to as much of a dilute ſolution of litmus, as a quantity of ſpirit of nitre, which contained $\frac{3}{10}$ths of a grain of real acid, and therefore I judged theſe 9 grs. to contain an exceſs of acid, amounting to $\frac{3}{10}$ths of a grain; and if 9 grs. contain ſuch an exceſs, then the whole ſolution muſt have contained an exceſs amounting to $5\frac{6}{10}$ths of a grain, deducting which from 31,38, we find the quantity of acid ſaturated by 100 grs. of ſilver to be 25,78 grs. In this manner I proceeded with moſt other metallic ſolutions. The vitriolic ſolutions of tin, wiſmuth, regulus of antimony, nickel, and regulus of arſenic, containing a large exceſs of acid, I ſaturated part of it with cauſtic volatile alkali before I tried them with the infuſion of litmus, and I uſed the ſame expedient with the nitrous ſolution of iron, lead, tin, and regulus of antimony, and all the marine ſolutions. The proportion of vitriolic and marine acid taken up by lead, ſilver, and mercury, I determined by computing the quantity of real acid neceſſary to precipitate theſe metals from their ſolutions in the nitrous acid; and of all the determinations theſe appeared to me to be the moſt exact. However, as all the vitriols of theſe metals are, though in a ſlight degree, ſoluble in the nitrous acid, I was obliged to rectify the reſult from other conſiderations, and the ſame neceſſity occurred with regard to the marine ſalts of lead and mercury.

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The result of these experiments was, that 100 grs. of each of these acids take up, at the point of saturation of each metallic substance, dephlogisticated to such a degree as is necessary for its solution in each acid, the quantities expressed in the following table, which denote their degree of affinity to each metal.

Table of the affinity of the three mineral acids to metallic substances.

100 grs.	Iron.	Copper.	Tin.	Lead.	Silver	Mercury.	Zinc.	Wismuth.	Nickel	Cobalt	Reg. of antimo.	Reg. of arsenic.
Vitriolic acid	270	260	138	412	390	432	318	250 310	320	360	200	260
Nitrous acid	255	255	120	365	375	416	304	290	300	350	194	220
Marine acid	265	265	130	400	420	438	312	250 320	275 310	370	198	290

Yet I cannot say that these numbers are *precisely* such as I could extract from my observations on the colour of the solution of litmus; for these indications are so precarious that I did not absolutely confide in them, but adjusted the numbers, as I thought other phenomena required. However, the deviations were not so considerable as to induce a doubt that metallic earths had not almost all a stronger affinity to the three acids than even fixed alkalies. Nevertheless, the common tables, which postpone metallic substances to all others, are in reality just; they only require a different denomination, being in fact tables of *precipitation* rather than of *affinity*, as far as they relate to metallic substances, expressing by their *order*, what metallic substances precipitate others from the different acids. But these precipitations are constantly the result of a double affinity and decomposition,

decompoſition, the *precipitating* metal yielding its phlogiſton to the *precipitated* metal, while the *precipitated* metal yields its acid to the *precipitant*. Nor has this eſcaped the ſagacity of Mr. BERGMAN, 2 N. Act. Upſ. 205. who has even confirmed it by experiments which I have repeated, and found exact. Thus, though copper, in its metallic form, precipitates ſilver and mercury from the nitrous acid with great eaſe, yet the calx of copper will precipitate neither. The ſuperior affinity of acids to metallic earths, in preference to alkalies and unmetallic earths, requiring further proof, I ſhall here demonſtrate it in a few inſtances, with regard even to thoſe metals which are commonly thought to have the leaſt affinity to acids. And, firſt, that the nitrous acid has a ſtronger affinity to ſilver than to fixed alkalies, appears by a curious experiment of Mr. MONNET's (Diſſolution des Metaux, p. 159.). If a ſolution of ſilver in nitrous acid be poured into a mixed ſolution of fixed alkali and common ſalt, the ſilver will be precipitated by the marine acid of the common ſalt, and not by the free alkali contained in the liquor, for a *luna cornua* is found. Now if the nitrous acid had a greater affinity to the free alkali than to the ſilver, it is evident, that the decompoſition would be wrought by the free alkali, and then the ſilver would be precipitated pure, and not in the ſtate of horn ſilver; but as it is precipitated in the ſtate of horn ſilver, it is plain, its precipitation was not effected by a ſingle but by a double affinity. From whence it alſo follows, that the marine acid has a greater affinity to ſilver than the nitrous has to fixed alkalies. I repeated this experiment with a ſolution of lead and alſo of mercury in the nitrous acid, and the reſult was ſimilar, horn lead and marine ſalt of mercury being formed.

With

With regard to mercury, the experiments of Mr. BAYEN are well known: he has ſhewn that vitriol of lead and ſub. corroſive can be deprived of no more than half of their acid even by cauſtic fixed alkalies, 3 ROZ. 293.

Again as to lead, if perfectly dry common ſalt be projected on lead heated to incandeſcence, the common ſalt will be decompoſed, and horn lead formed, 1 MARGRAF. 35 and 38. Nor can this be attributed to the volatilization of the acid by heat; for the alkali is as fixed as the lead, and muſt therefore be cauſed by the greater affinity of the calx of lead, to which, when dephlogiſticated, the acid can unite. Mr. SCHEELE informs us, that if a ſolution of common ſalt be digeſted with litharge, the common ſalt will be decompoſed, and a cauſtic alkali produced, SCHEELE on Fire, p. 175. He alſo decompoſes common ſalt by ſimply letting its ſolution ſlowly paſs through a funnel filled with powdered litharge. * Mr. TURNER daily decompoſes common ſalt by means of litharge. Mr. SCHEELE alſo decompoſes marine ſelenite, by means of litharge, through ſimple mixture, without the aſſiſtance of heat, and the calcareous earth is ſeparated in a cauſtic ſtate; which ſhews that this ſalt is decompoſed by the ſingle ſuperior affinity of the metallic calx to the marine acid, SCHEELE on Fire, p. 174.

That acids have leſs affinity to volatile alkalies than to ſeveral metallic ſubſtances appears in ſundry inſtances. Horn ſilver is ſoluble in volatile alkalies, as is well known. Now, if this ſolution be triturated with four times its weight of mercury, the marine acid will combine with the mercury, and not with the volatile alkali; for a *mercurius dulcis*, and not a ſal ammoniac, will be formed, as Mr. MARGRAF has ſhewn, 1 MARGR. 286. If two parts ſal ammoniac and one of filings of iron be

* SCHEFFER Chymiſche Foceles, § 59.

triturated

triturated together, the ſmell of the volatile alkali will immediately be perceived *; or if, inſtead of iron, minium, or diaphoretic antimony, or zinc, be uſed, that ſmell is perceived as ſoon as they are mixed, 9 Mem. Scav. Etrang. p. 575. MONNET, Diſſ. Met. 209. But it will naturally be aſked, how then it happens, that all metallic ſolutions are precipitated by alkalies and earths? The anſwer is eaſy; all metallic ſalts are held in ſolution by an exceſs of acid. If alkalies and earths did nothing more than abſorb this exceſs of acid, a precipitation ought to take place; but they do ſtill more, for they take up the greater part even of the proportion of acid neceſſary to ſaturate the metallic earth, and this they are enabled to do by means of a double affinity; for during the ſolution of metals, only a comparatively ſmall part of the phlogiſton eſcapes out of the ſolution, the remainder is retained by the compound of acid and calx: when, therefore, an alkali or earth is added to ſuch a ſolution, the phlogiſton quits the acid, and re-combines with the calx, while the greater part of the acid unites to the precipitant. Notwithſtanding this great affinity of metallic earths to acids, ſalts, whoſe baſis is a fixed alkali or earth, are in few inſtances decompoſed by metals or their calces, by reaſon of the inability of the acids while combined with theſe baſis, and thereby deprived of a great part of their ſpecific fire, to volatilize the phlogiſton combined with the metallic earths, which muſt neceſſarily be expelled before an acid can combine with them. And as to metallic calces, they are generally combined with fixed air, which alſo muſt be in part expelled.

But ammoniacal ſalts, containing much more fire (for they abſorb fire during their formation) for that reaſon act much more powerfully on metals. Allowing then the affinity of the

* MONNET, Diſſol. Met. 71.

mineral acids to metallic ſubſtances to be as above, all double decompoſition in which only ſalts, containing theſe acids united to alkaline terrene or metallic baſes, are concerned, admit of an eaſy explanation; nay, I am bold to ſay, they cannot otherwiſe be explained. Thus if a ſolution of tartar vitriolate, and of ſilver in the nitrous acid, be mixed in proper proportions (which is always to be underſtood), nitre and vitriol of ſilver will be formed, and this latter for the moſt part precipitated.

Quieſcent affinities.		Divellent affinities.	
Nitrous acid to ſilver	375	Nitrous acid to vegetable alkali	215
Vitriolic acid to vegetable alkali	215	Vitriolic acid to ſilver	390
Sum of the quieſcent	590	Sum of the divellent powers	605

So alſo if, inſtead of a ſolution of tartar vitriolate, that of GLAUBER's ſalt, or of vitriolic-ammoniac, or ſelenite, Epſom, or allum, be uſed; for in all theſe caſes the balance is conſtantly in favour of the divellent powers, yet the ſolutions of ſelenite and allum produce but a ſlight precipitation.

I alſo found, that the ſolution of ſilver is precipitated by the vitriolic ſolutions of iron, copper, tin, and probably by many other vitriolic ſolutions, if for no other reaſon at leaſt for this, that they conſtantly contain an exceſs of acid; but if a ſaturate ſolution of ſilver be mixed with a very ſaturate ſolution of lead or mercury in the vitriolic acid, the ſilver will not be precipitated, as I have obſerved; and in both caſes the balance is in favour of the quieſcent affinities.

The nitrous ſolution of ſilver is alſo decompoſed, and the ſilver precipitated by all *marine* neutral ſalts, whether the baſis be alkaline, terrene, or metallic, as I have experienced, and

theſe decompoſitions are conſtantly indicated by the balance of affinities exhibited as above.

In the ſame manner ſilver is precipitated from the *vitriolic* acid by *marine* neutral ſalts, whether their baſis be alkaline, terrene, or metallic, as I have found on trial, and as the balance of affinities requires.

The nitrous ſolution of lead is alſo decompoſed, and the lead for the moſt part precipitated (unleſs the ſolution be very dilute) in the form of vitriol of lead by all the *vitriolico* neutral ſalts; and alſo by all the *marino* neutral ſalts, except marine ſalt of ſilver, which only precipitates it by virtue of its exceſs of acid.

The marine ſolution of lead is decompoſed by all *vitriolico* neutral ſalts, except ſelenite and vitriol of nickel, which can only precipitate it by virtue of an exceſs of acid.

The nitrous ſolution of mercury is alſo decompoſed, and the mercury for the moſt precipitated in the form of vitriol of mercury by all vitriolic neutral ſalts, except vitriol of lead, which can only decompoſe it by an exceſs of acid.

Nitrous ſolution of mercury is alſo decompoſed by *marino* neutral ſalts, except the marine ſalt of ſilver and lead, which can only affect it by an exceſs of acid.

Vitriol of mercury is alſo decompoſed by marino neutral ſalts, which decompoſition is alſo apparent by expoſing the antagoniſt powers; yet a precipitation does not always appear as I have remarked, particularly when marine allum is uſed, which I attribute to the facility with which a ſmall quantity of the marine ſalt of mercury is ſoluble in an exceſs of acid. Marine ſalt of ſilver decompoſes vitriol of mercury, only through its exceſs of acid.

Hence we see why horn silver can never be reduced by fixed alkalies without loss, as Mr. MARGRAAF has shewn, I MARGR. 277.; nor could it be decomposed at all, but that the action of heat helps that of the alkali.

If to a solution of sublimate corrosive oil of vitriol be added, a precipitation will appear; but, as Mr. BERGMAN well remarks, this does not proceed from a decomposition, but from a substraction of the water necessary to keep the sublimate dissolved.

If to a solution of vitriol of iron some nitrous acid be added, it immediately becomes turbid, because the nitrous acid dephlogisticates the calx of iron too much, but the addition of more acid restores the transparency, as the dephlogisticated calx is still soluble by a greater quantity of acid. I omit a number of other curious phenomena, which are explicable on these principles.

I have assigned in the foregoing table two different affinities to the vitriolic acid with regard to wismuth, and also to the marine acid with regard to nickel and wismuth. The first shews that which these acids bear to those metals, when dephlogisticated only by solution in those acids. The second number, that which the acids bear to them when more dephlogisticated, as they are, when dissolved in the nitrous acid. On the other hand, all the acids have less affinity to the calces of iron, zinc, tin, and antimony, when they are dephlogisticated to a certain degree; but as I could give no criterion of this dephlogistication, I did not attempt to indicate the diminution it causes in the affinities of acids.

Of the precipitation of metals by each other from the mineral acids.

I am now come to the laſt point of my inquiry, and the moſt difficult to be ſet forth with that degree of preciſion which I have been enabled to attain in the former parts; for, in the firſt place, it is neceſſary to find the quantity of phlogiſton in each of them, not only in general, but according to their various degrees of dephlogiſtication by each of the acids. In this laſt particular I cannot aſſert that I have attained any thing like a certainty, yet I hope what I advance may not be uſeleſs to chymical readers, as it is not altogether groundleſs, as it contradicts no chymical fact, but, on the contrary, is agreeable to many, and affords a ready ſolution of all the phenomena.

Of the abſolute quantity of phlogiſton in metals.

The proportion of phlogiſton in metallic ſubſtances relatively to each other has been inveſtigated in ſo maſterly a manner by Mr. BERGMAN, that I lay it down as the ground of my inquiries. After his diſcovery all that remained was to find the abſolute quantity of it in any one metal, for then, by an eaſy calculation, it may be determined in all the reſt. The ſubſtance I choſe for this purpoſe was regulus of arſenic, as being moſt capable of dephlogiſtication by nitrous acid, though not altogether ſo.

From 100 grs. of regulus of arſenic, diſſolved in dilute nitrous acid, as already ſeen, 102 cubic inches of nitrous air and $\frac{4}{10}$ths are obtained, barometer at 30°, thermometer at 60°. I muſt add, that I made the experiment on 5 grs. only, ſo that the calculation relates only to the quantity of air which 100 grs.

grs. *ſhould* give. I repeated the experiment three times with the ſame ſucceſs. I attempted getting more air from the reſiduum left by a gentle evaporation, but though freſh ſpirit of nitre grew red with it, the quantity of air was quite inconſiderable.

Now this quantity of nitrous air contains 6,86 grs. of phlogiſton, according to the calculation to be ſeen in my former paper; and hence I conclude, that 100 grs. of regulus of arſenic contains 6,86 grs. of phlogiſton. This regulus was made by Mr. WOLFE, and perfectly bright.

Hence the relative proportion of phlogiſton in metals being, as found by Mr. BERGMAN, and ſet forth in the firſt column of the following table, the abſolute quantity will be as ſhewn in the ſecond column.

		Relative quantity of phlogiſton.	Abſolute quantity.
100 grs.	Gold	394	24,82
	Copper	312	19,65
	Cobalt	270	17,01
	Iron	233	14,67
	Zinc	182	11,46
	Nickel	156	9,82
	Regulus of antimony	120	7,56
	Tin	114	7,18
	Regulus of arſenic	109	6,86
	Silver	100	6,30
	Mercury	74	4,56
	Wiſmuth	57	3,59
	Lead	43	2,70

This point being, as I conceived, of ſome importance, I endeavoured to aſcertain it ſtill further by other experiments: and

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as ſilver loſes a certain quantity of phlogiſton, which eſcapes and ſeparates from it during its ſolution in nitrous acid, I conceived, that if the ſolution was expoſed to nothing from which it could re-obtain phlogiſton, and thus diſtilled to dryneſs, and intirely ſeparated from the acid, as much ſilver ſhould remain unreduced as correſponded with the quantity of phlogiſton loſt by it. And if this quantity of phlogiſton correſponded with that aſſigned to ſilver in the foregoing table, that then this table was juſt.

For this purpoſe I diſſolved 120 grs. of clean filings of ſtandard ſilver in dilute dephlogiſticated nitrous acid, and obtained from it 24 cubic inches of nitrous air. This ſolution I gently evaporated to dryneſs; by the evaporation I found a little of the ſilver volatilized, but not more than a quarter of a grain. I then diſtilled the dry reſiduum, and kept it an hour in almoſt a white heat in a coated green glaſs retort. During the diſtillation abundance of the nitrous acid paſſed off, a green and white ſublimate aroſe in the neck of the retort, and ſome paſſed even into the receiver. When all was cold I broke the retort, the inſide of which was penetrated into its very ſubſtance with a yellow and red tinge, and partly covered over with an exceeding fine ſilver powder, which could ſcarcely be ſcraped off. The remainder of the ſilver was perfectly white and free from acid, but not melted into a button, and when collected weighed 94 grs.; therefore 26 grs. were loſt, that is, were ſublimed or vitrified; but of theſe 26 grs. 9 grs. were copper (for 100 grs. ſtandard ſilver contain 7½ of copper); therefore, only 17 grs. of pure ſilver remained unreduced, being either volatilized or vitrified. The whole quantity of pure ſilver in 120 grs. of ſtandard ſilver amounts to 111 grs.; then if 111 grs. of pure ſilver loſe 17 by reaſon of its

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loſs of phlogiſton, 100 grs. of pure ſilver ſhould loſe 15,3; and by the above table 15,3 grs. of ſilver ſhould contain 0,945 of a grain of phlogiſton. Let us now ſee whether this quantity of phlogiſton correſponds with that which 100 grs. of pure ſilver really loſe by ſolution in nitrous acid. 100 grs. of pure ſilver afford, as already ſaid, 14 cubic inches of nitrous air, which, by my computation, contain 0,938 of a grain of phlogiſton, which differs from 0,945 only by $\frac{7}{1000}$. The unreduced part of the ſilver was 15,3 grs.; and, by calculating what it ſhould be by reaſon of the loſs of the phlogiſton contained in the nitrous air, it would amount to 14 and $\frac{9}{10}$ths of a grain, a difference certainly immaterial.

In this experiment, only as much of the ſilver ſublimed as could not regain phlogiſton; the remainder regained it from the nitrous air abſorbed by the ſolution, and alſo from that which remained united to the acid and calx. If this were not ſo, I do not ſee why the whole of the ſilver would not ſublime.

Again: Dr. PRIESTLEY having ſeveral times diſſolved mercury in the nitrous acid, and revivified it by diſtilling over that acid, conſtantly found a conſiderable proportion of it unreduced. To try whether that proportion correſponded with my calculation, I have examined the experiment which he made with moſt care, and which is to be found in his 4th vol. p. 262. We there find, that having diſſolved 17 dwts 13 grs. = 321 grs. of mercury in nitrous acid, 1½ dwt. that is, 36 grs. remained unreduced. Now, according to my calculation, 56 grs. ſhould remain unreduced; for 100 grs. of mercury afford 12 cubic inches of nitrous air; therefore 321 grs. ſhould afford 38,52, which contain 2,58 grs. of phlogiſton, and if (according to the table) 4,56 grs. of phlogiſton be neceſſary to metallize 100 grs. of mercury, 2,58 will be neceſſary to metallize 56 grs.

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of mercury: and I am ſatisfied, from my own trials, that more than 50 grs. would be found unreduced if dephlogiſticated nitrous acid had been uſed in diſſolving the mercury, and the ſolution performed with heat and a ſtrong acid; but that which Dr. PRIESTLEY uſed, being the red or yellow ſort, already contained much phlogiſton, which contributed to the revivification of a larger quantity of mercury than would otherwiſe be found. It is true, that Dr. PRIESTLEY afterwards revivified a great part of what originally remained unreduced; but this happened after it was for ſome time expoſed to the free air, from which the calces of the perfect metals always attract phlogiſton, as is evident in luna cornua, which blackens on expoſure to the air, and hence alſo proceed the reductions operated by Mr. BAYEN.

But Dr. PRIESTLEY, to whoſe luminous experiments chymiſtry is already ſo much indebted, has been ſo obliging as to furniſh me with ſome which tend more directly to elucidate the preſent queſtion.

In one experiment he found that nearly 5 dwts. of minium, from whence all its air was extracted, that is, about 118 grs. abſorbed 40 oz. meaſures of inflammable, that is, 75,8 cubic inches = 2,65 grs. of phlogiſton, and were then reduced: then 100 grs. of minium ſhould require for their reduction nearly 2,25 grs. of phlogiſton. In another experiment, made with more care, he found, that 480 grs. of minium abſorbed 108 oz. meaſures of inflammable air; according to this then 100 grs. of minium require for their reduction 1,49 grs. of phlogiſton; and in two other experiments he found this quantity ſtill leſs. Upon which I remark, firſt, that the whole of the minium was not dephlogiſticated; for, beſides that it is never throughout equally calcined, much of it muſt have been

reduced

reduced during the expulſion of its air; ſecondly, that the quantity of the phlogiſton in the inflammable air may have been greater as this varies with its temperature, and the weight of the atmoſphere; ſo that upon the whole theſe experiments confirm the reſults expreſſed in the table.

Of the affinity of metallic calces to phlogiſton.

That inflammable air or phlogiſton is condenſed to a very conſiderable degree by uniting to any metallic ſubſtance, ſo that its ſpecific gravity is not only equal, but much ſuperior, to that of the metallic earth with which it combines, may eaſily be concluded from the example of fixed air, which, by uniting to calcareous earth, acquires a ſpecific gravity equal to that of gold; and hence, that metallic earth which condenſes phlogiſton moſt, and in greateſt quantity, uniting to it moſt cloſely, may be ſaid to have the greateſt affinity to it; ſo that if we could find the ſpecific gravity of a calx perfectly pure, both from phlogiſton and fixed air, we could, by comparing its denſity with that of the ſame calx when metallized, know the denſity which phlogiſton acquires by its union with ſuch calx; but to procure ſuch calces hath hitherto proved impoſſible, as, during their dephlogiſtication, they combine with fixed air, or ſome particles of their menſtruum; and hence their abſolute weight is increaſed, though their ſpecific gravity be ſomewhat diminiſhed. From this laſt circumſtance it appears, that the ſpecific gravity of calces differs much leſs from that of their reſpective metals than does the ſpecific gravity which the phlogiſton acquires by its union with thoſe calces, from that which it poſſeſſes in its uncombined ſtate; in the ſame manner as the denſity of quick-lime differs much leſs from that of lime-ſtone, than does the denſity which fixed air acquires by its

union with quick-lime from that which belongs to it in its aërial ſtate; and hence, inſtead of deducing the quantity of affinity of metallic calces to phlogiſton from the following propoſition, *viz.* that *the affinity of metallic calces to phlogiſton is in a compound ratio of its quantity and denſity in each metal*, I am obliged to deduce it from this other, *viz.* that *the affinity of metallic calces to phlogiſton is directly as the ſpecific gravity of the reſpective metals, and inverſely as the quantity of calx contained in a given weight of thoſe metals.* This latter propoſition is an approximation to the former, founded on this truth, that *the larger the quantity of phlogiſton in any metal is, the ſmaller is the quantity of calx in a given weight of that metal*; and that *the denſity which the phlogiſton acquires, is as the ſpecific gravity of the metal.* This latter propoſition, however, is not exactly true, for this denſity is much greater; yet it is the neareſt approximation I can make, and its defect is ſenſible only with regard to thoſe metals which contain a conſiderable quantity of phlogiſton, *viz.* gold, copper, cobalt, and iron: with regard to the reſt it is of no importance.

Then the ſpecific gravity of metals being as repreſented in the firſt column of the following table, the affinity of their calces to phlogiſton will be as is ſhewn in the ſecond column. The third column expreſſes theſe affinities in numbers homogenous with thoſe which expreſs the affinities of acids with their baſis.

Gold

	Specific gravity.		Affinity of the calces to phlogiston.
Gold	19	0,25	1041
Mercury	14	0,147	612
Silver	11,091	0,118	491
Lead	11,33	0,116	483
Copper	8,8	0,109	454
Wismuth	9,6	0,099	412
Cobalt	7,7	0,092	383
Iron	7,7	0,090	375
Regulus of arsenic	8,31	0,089	370
Zinc	7,24	0,0817	340
Nickel	7,33	0,0812	338
Tin	7,	0,075	312
Regulus of antimony	6,86	0,074	308

Here we see, that the calx of lead has a greater affinity to phlogiston than the calces of any of the imperfect metals, and hence its use in cuppellation; for after it has lost its own phlogiston, it extracts that of the base metals, and thus promotes their calcination and vitrification.

Though the numbers in the second column express tolerably well the greater or lesser affinity of metallic calces to phlogiston, yet they have this inconvenience, that they are not homogenous with those that express the affinities of acids to other bases, which limits their use to a narrow compass, they being, on that account, incomparable with those that express the affinities of acids: I therefore endeavoured to find a coincidence between them in some one instance, in order to reduce them to the same standard, as will be seen in the next paragraph.

Of the affinity of the vitriolic acid to phlogiſton in ſulphur.

According to the principle above laid down, this affinity is in a compound ratio of the quantity of phlogiſton taken up by 100 grs. of the vitriolic acid and of the denſity it acquires by its union with the acid. Now 100 grs. of ſulphur contain 59 of acid and 41 of phlogiſton, and the ſpecific gravity of ſulphur is 2,344; therefore, the loſs of weight of ſulphur in water = $\frac{100}{2,344} = 42,66$ grs. the loſs of weight of the acid part of the ſulphur is $\frac{59}{4,226} = 13,96$ grs.; therefore, the remainder of the loſs of ſulphur is the loſs of the phlogiſtic part = 28,70 grs.; then the abſolute weight of the phlogiſton being 41 grs. its denſity will be $1,429 = \frac{41}{28,7}$; and ſince 100 grs. of vitriolic acid take up 70 of phlogiſton its affinity will be $1,429 \times 70 = 100$. But if the affinity of the vitriolic acid to phlogiſton in ſulphur had been ſought in the ſame manner with the affinity of metallic calces to phlogiſton, the quantity would be the ſame, though the expreſſion of that quantity would be different, as relating to a different ſtandard; for by that method the affinity would be directly as the denſity of the phlogiſton, and inverſely as the quantity of vitriolic acid contained in 100 grs. of ſulphur, that is, $\frac{1,429}{59} = ,024$; therefore, this expreſſion anſwers to, and is equivalent to the former, *viz.* 100. By this means I formed the quantities expreſſed in the third column, which are homogenous to thoſe which expreſs the affinities of acids to their baſes. Thus, the affinity of the calx of gold to phlogiſton is 1041, for :: ,024 . 100 :: ,25 . 1041, &c.

The

The third point neceſſary for the explanation of the phenomena attending the ſolution of metals, and their precipitation by each other, is to determine the proportion of phlogiſton which they loſe by ſolution in each of the acids, and the affinity which their calces bear to the part ſo loſt. I have not been able to determine this by any direct experiment; for though I might determine the part which eſcapes in the form of air, yet I could not that which is equally ſeparated from the metal, but retained in the ſolution; yet from various collateral conſiderations I am induced to think, the proportion of phlogiſton, ſeparated from the metals by the different acids, is, at a medium, as expreſſed in the following table.

	Iron.	Copper.	Tin.	Lead.	Silver.	Mercury.	Zinc.	Wiſmuth.	Cobalt.	Nickel	Regulus of Antimony.	Regulus of arſenic.
By vitriolic acid	2/3	8/10	7/10	98/100	Intire	87/100	85/100	85/100	93/100	Intire	97/100	96/100
By nitrous acid	2/3	83/100	7/10	88/100	Intire	9/10	99/100	97/100	Intire	Intire	Intire	99/100
By marine acid	43/100	57/100	3/10	6/10			6/10	7/10	36/100	8/10	14/100	4/10

On this ſuppoſition the affinity of the calces to the deficient part of their phlogiſton may eaſily be calculated; for they may be conſidered as acids whoſe affinity to the deficient part of their baſis is as the ratio which that part bears to the whole, as already ſaid. Thus the affinity of iron, thoroughly deprived of its phlogiſton, being 375, as it loſes $\frac{2}{3}$ds of its phlogiſton by ſolution in the vitriolic acid, the affinity of iron to theſe two-thirds is $\frac{2}{3}$ds of its whole affinity, that is, $\frac{2}{3}$ds of $375 = 250$.

Theſe affinities, together with thoſe of the three acids to the ſeveral calces, are repreſented together in the following table.

Vitriolic

	Iron.	Copper.	Tin.	Lead.	Silver	Mercury.	Zinc.	Wiſmuth.	Cobalt.	Nickel.	Reg. of antimo.	Reg. of arſenic.
Vitriolic acid	270	360	138	412	390	432	318	250.310	360	320	200	260
Calx to phlog.	250	360	218	483	491	532	298	350	300	338	300	320
Nitrous acid	235	255	120	303	375	416	304	290	350	300	194	255
Calx to phlog.	250	363	218	424	491	552	337	400	383	338	308	366
Marine acid	205	265	150	400	420	438	312	250.320	370	275.310	198	290
Calx to phlog.	165	260	104	290	491	500	200	280	360	265	240	300

The affinities of calces to phlogiſton are taken at a medium; for almoſt all metallic ſubſtances are capable of greater or leſſer dephlogiſtication, according to the ſpecies, concentration, and dephlogiſtication of their menſtruum. The more they are dephlogiſticated, the greater their affinity to phlogiſton; and, in general, the leſs their affinity to the mineral acids. Yet there is a point of dephlogiſtication at which the attraction of acids to the calces is ſtrongeſt: thus the vitriolic acid attracts biſmuth moſt ſtrongly after it has been dephlogiſticated by the nitrous acid; and the marine acid attracts both biſmuth and nickel more powerfully, when dephlogiſticated by the nitrous or vitriolic acids.

From theſe *data* we may eaſily conceive, in moſt caſes, what will happen on putting one metal into the ſolution of another.

Thus, if a piece of copper be put into a ſaturate ſolution of ſilver, the ſilver will be precipitated; for the balance is in favour of the divellent powers.

Quieſcent

Quieſcent affinities.		Divellent affinities.	
Nitrous acid to ſilver -	375	Nitrous acid to copper -	255
Calx of copper to phlogiſton	363	Calx of ſilver to phlogiſton	491
Sum of the quieſcent affinities	738	Sum of the divellent —	746

The ſolutions muſt be nearly ſaturate, elſe a large quantity of the added metal will be diſſolved by the free acid, before any precipitation can appear; yet it muſt not be intirely ſaturate, at leaſt in ſome caſes, as will preſently be ſeen.

I ſaid in *moſt caſes*, becauſe in ſome, particularly where mercury, biſmuth, cobalt, regulus of antimony or arſenic, are uſed, another power intervenes which has not yet been fully inveſtigated, *viz.* the attraction of calces to each other, which I ſhall occaſionally mention.

It is worthy of obſervation, that the precipitating metals are more dephlogiſticated by this means than by direct ſolution in their reſpective menſtruums, and are even diſſolved by menſtruums that would not otherwiſe affect them; becauſe their phlogiſton is torn from them by two powers inſtead of one: thus, though copper be directly ſoluble in the vitriolic acid, only when this acid is concentrated and heated to a great degree, yet if a piece of copper be put into a dilute cold ſolution of ſilver or mercury in the vitriolic of acid, or even into a dilute ſolution of iron, expoſed to the open air, it will be diſſolved; a circumſtance which juſtly excited the wonder of Mr. MARGRAAF and Mr. WENZEL, who did not apprehend the theory of it: and hence we ſee how vitriol of copper may be formed by nature, and why it always contains a mixture of iron.

Of ſolutions in the vitriolic acid.

This acid diſſolves iron and zinc, without the aſſiſtance of heat; becauſe its affinity to their calces is greater than the affi-

nity which theſe calces bear to that portion of phlogiſton which they muſt loſe before they can unite to the acid, as may be ſeen by inſpecting the table; but all other metallic ſubſtances unite to this acid only where it is concentrated and heated.

Of ſolutions in the nitrous acid.

The nitrous acid has leſs affinity to all metallic ſubſtances than either the vitriolic or marine. It has alſo leſs affinity to them than they have to that portion of phlogiſton which they muſt loſe before they can unite to it; yet it diſſolves them all (gold and platina excepted) even without the aid of heat becauſe it unites itſelf to phlogiſton unleſs too dilute; and the heat produced by its union with phlogiſton is ſufficient to promote the ſolution.

But if it be too concentrated, it will not act either on lead or ſilver, without the aſſiſtance of heat, as BOYLE and BOERHAAVE have remarked *; for the difference betwixt its affinity to theſe metals, and that of theſe metals to the portion of phlogiſton which they muſt loſe before they can unite to it, is very great; and when it is very concentrated, the liquor does not contain fire enough to throw the phlogiſton and it into an aërial form, and reduce the ſolid to a liquid; the ſame would probably be obſerved with regard to mercury, if it had not been already in a liquid ſtate. STAHL has alſo remarked, that it produces very little heat in diſſolving ſilver, and none in diſſolving lead or mercury †. This is eaſily explained, now that we know that ſilver contains but little phlogiſton, and lead much leſs, the heat being evidently produced, according to the

* 1 SHAW's BOERH. 508. 2 SHAW's BOYLE 335.

†. STAHL ſur les ſels 168.

law discovered by Dr. CRAWFORD, by the union of phlogiston to the acid, for metallic calces produce no heat. As to mercury, the observation is not exact, for its solution is accompanied with heat, as Mr. LAVOISIER has observed, 1 LAVOIS. 248.

Of solutions in the marine acid.

This acid is known to dephlogisticate metals less than any other. Where the portion of phlogiston, necessary to be separated, is more strongly attracted than the acid itself, it can operate no solution, or at least very slowly, without the aid of heat; nor even where the attraction of acid is stronger to the calx than that of the portion of phlogiston it separates, if the proportion of acid to such calx be very small; because so small a quantity of acid does not contain fire enough to volatilize the phlogiston; and hence heat is necessary for the solution of lead in this acid. The dephlogisticated acid acts more powerfully.

Of precipitations of and by iron.

The mutual precipitations of iron and copper from the vitriolic acid by each other, have been well explained in a general manner by Mr. MONNET and Mr. BERGMAN; I shall here shew the reason of these precipitations more distinctly.

If a piece of copper be put into a saturate solution of iron, fresh made, no precipitation will happen, nor will any of the copper be dissolved in twelve hours, nor even in a longer time, if the access of air to the solution be prevented; but if the solution be exposed to the open air, the addition of a volatile alkali will shew the copper to have been acted upon in 24 hours, or sooner if heat be applied, and a calx of iron is precipitated. The operation of the affinities in the first case is as follows.

Quieſcent.		Divellent.	
Vitriolic acid to calx of iron	270	Vitriolic acid to copper -	260
Copper to its phlogiſton -	360	Calx of iron to phlogiſton	250
Sum - - -	630	Sum - - -	510

Hence, in this caſe, no decompoſition can happen; but in the ſecond caſe, much of the phlogiſton of the ſolution of iron having eſcaped, the affinity of the calx of iron to acid is diminiſhed, and that to phlogiſton is increaſed, and therefore the quieſcent affinities may be ſuppoſed,

		and the divellent.	
Vitriolic acid to the calx of iron	240	Vitriolic acid to copper -	260
Copper to its phlogiſton -	360	Calx of iron to phlogiſton	370
	600		630

But from the increaſed affinity of the calx of iron to phlogiſton it might be inferred, that as the iron recovers its phlogiſton, the acid ſhould re-act upon it and quit the copper; and this would certainly happen, if it recovered its phlogiſton in ſufficient quantity, but the acceſs of air and heat prevents its retaining it, at leaſt in ſufficient quantity.

This increaſed affinity of the calx of iron to phlogiſton is not a mere ſuppoſition; for, if into a ſolution of iron, ſo far dephlogiſticated as to refuſe to cryſtallize, ſome freſh iron be put, the impoveriſhed calx will re-attract ſo much of the phlogiſton given out during the ſolution of the freſh iron, that it will now afford cryſtals, as Mr. MONNET has obſerved in his excellent Treatiſe on Vitriolization. The diminiſhed attraction of the calx of iron to acids is alſo evident from this experiment,

and alſo from the neceſſity of adding more acid to a turbid ſolution of iron, in order to re-eſtabliſh its tranſparency. The calces of copper alſo precipitate a dephlogiſticated ſolution of iron, as they ſhould, the affinity of the acid to ſuch calx of iron being 240, and that to copper being 260. With regard to the ſolution of iron in nitrous acid, the ſame thing happens; but as this ſolution contains a large exceſs of acid, a portion of copper is diſſolved even before any of the iron is precipitated.

With regard to a ſolution of iron in the marine acid, though expoſed to the open air, copper precipitates nothing from it in 24 hours.

But if a clean piece of iron be put into a ſolution of copper in the vitriolic acid, the copper is immediately precipitated; for here the quieſcent and divellent affinities exhibited in the firſt ſcheme are reverſed, the quieſcent becoming the divellent, and *vice verſâ*. It is needleſs to add, that copper is in the ſame manner precipitated by iron from the nitrous and marine acids.

Hence the practice of extracting copper from ſome mineral waters by means of iron. Theſe waters, therefore, furniſh afterwards, by evaporation, vitriol of iron; but it is remarkable, that this vitriol is much paler than the common, and leſs fit for dying, 2 SCHLUTTER 507. The reaſon of which is, that it is more dephlogiſticated, not only becauſe old iron is chiefly uſed, but becauſe copper, containing more phlogiſton than an equal weight of iron, deprives it of more of its phlogiſton than it would loſe if barely diſſolved in the vitriolic acid.

Caſt iron, according to SCHLUTTER, will ſcarcely precipitate a ſolution of copper; and in effect Mr. BERGMAN has found that it contains leſs phlogiſton than bar iron.

I have always found ſilver to be eaſily precipitated from its ſolution in the nitrous acid by iron. The ſum of the quieſcent affinities being 625, and that of the divellent 746; yet Mr. BERGMAN obſerved, that a very ſaturate ſolution of ſilver was very difficultly precipitated, and only by ſome ſorts of iron, even though the ſolution was diluted, and an exceſs of acid added to it *; the reaſon of this curious phenomenon appears to me deducible from a circumſtance firſt obſerved by Mr. SCHEELE, in diſſolving mercury, namely, that the nitrous acid, when ſaturated with it, will take up more of it in its metallic form †. The ſame thing happens in diſſolving ſilver in the nitrous acid in a ſtrong heat; for, as I before remarked, the laſt portions of ſilver thrown in afford no air, and conſequently are not dephlogiſticated. Now this compound of calx of ſilver, and ſilver in its metallic form, may well be unprecipitable by iron, the ſilver, in its metallic form, preventing the calx from coming in contact with the iron, and extracting phlogiſton from it; and hence alſo, iron has ſometimes been obſerved not to precipitate a ſolution of mercury in this acid ‡.

It has been long thought, that iron may be precipitated from acids by zinc, though NEWMAN long ago denied it; but Mr. BERGMAN has ſatisfactorily cleared up this point, by ſhewing that zinc cannot precipitate iron from the vitriolic acid, until the ſolution of iron loſes part of its phlogiſton. With regard to the nitrous acid, I found, that zinc does not precipitate iron; but, on the contrary, iron precipitates zinc; but in a ſhort time the acid re-diſſolves the zinc, and lets fall the iron, which evidently proceeds from the too great dephlogiſtication of the calx of iron. But zinc precipitates iron from the marine,

* Diſſert. de Phlog. Quantitate in Metal. p. 6.

† 39 SUENSK. Handling, p. 70.

‡ 2 CRELL. Nev. Entdeck. p. 266.

though

though with difficulty; for after 24 hours the galls ſtill ſtruck a black. I ſhould alſo add, that iron does not precipitate zinc from the vitriolic acid.

Moſt metallic ſubſtances, precipitated by iron from the nitrous acid, are in ſome meaſure re-diſſolved ſhortly after, as the nitrous acid ſoon dephlogiſticates the iron too much, then lets it fall, and re-acts on the other metals and re-diſſolves them.

The precipitation of the argillaceous earth from allum by iron is owing to the exceſs of acid in the allum which firſt dephlogiſticates the iron; and when this is dephlogiſticated, it attracts the acid more ſtrongly. Earth of allum, on the other hand, precipitates iron when the ſolution of iron is dephlogiſticated by heat. It may alſo produce this effect by depriving iron of its exceſs of acid which keeps it in ſolution.

Of precipitations of and by copper.

When ſilver is diſſolved in the nitrous acid, and a piece of copper is put into the ſolution, it ſometimes happens, that the ſilver is not precipitated, as Dr. LEWIS has obſerved *. This happens either when the nitrous acid is ſuperſaturated with ſilver having taken up ſome in its metallic form, as already obſerved; or when the ſilver is not much dephlogiſticated, for then its affinity to phlogiſton, which is the principal cauſe of its precipitation, is leſs than 491; therefore, the remedy is to heat it and add more acid, by which it is dephlogiſticated further. However, the nitrous acid always retains a little ſilver. SCHLÜTTER. 362. Hiſt. Mem. Par. 1728.

It is commonly ſaid, that if filings of copper be put into a boiling ſolution of allum, vitriol of copper will be found, and

* Commercium Philoſ. p. 157.

the

the earth of allum precipitated. If this were true it would be very ſurpriſing, as copper is ſoluble, even with the aſſiſtance of heat, only in the concentrated vitriolic acid. Hence I made the experiment, and found, that after 20 hours boiling not the ſmalleſt particle of copper was diſſolved, for the colour of the ſolution was not altered by volatile alkalies: and though the allum was precipitated, it ſtill retained its ſaline form, ſo that it loſt only its exceſs of acid in this experiment.

Of precipitations of and by tin.

Tin is not precipitated, in its metallic form, by any metallic ſubſtance; and the reaſon is, becauſe its precipitation is not the effect of a double affinity, but of the ſingle greater affinity of its menſtruum to every other metallic earth. Metals that are precipitated from the nitrous acid by tin, are afterwards re-diſſolved, becauſe the acid ſoon quits the tin, it becoming too much dephlogiſticated.

Of precipitations of and by lead.

Metals diſſolved in the vitriolic and marine acids, and precipitable by lead, according to the indication of the balance of affinities, are yet ſlowly precipitated, becauſe the firſt portions of lead that are diſſolved form ſalts of difficult ſolution, which cover its ſurface, and protect it from the further action of the acid; and yet it contains ſo little phlogiſton, that a great deal of it muſt be diſſolved before it gives out enough to precipitate the diſſolved metals.

Mr. BERGMAN obſerved, that a very ſaturate ſolution of lead is difficultly, if at all, precipitable by iron. Does not this alſo ariſe from ſome lead being taken up in its metallic form? Iron does not precipitate lead from the marine acid, though a pre-

precipitate appears; for this precipitate ſtill retains the marine acid: on the contrary, lead precipitates iron from this acid, though very ſlowly.

Of precipitations of and by mercury.

Though the difference betwixt the quieſcent and divellent powers be very ſmall, yet mercury is quickly precipitated from the vitriolic acid by copper; becauſe the attraction of the calx of mercury to phlogiſton is very ſtrong, and a very ſmall proportion of that contained in copper is ſufficient to revive it.

Silver does not precipitate mercury from the vitriolic acid, unleſs it contains copper, and then it does precipitate it; yet if ſilver and turpeth mineral be diſtilled, the mercury will paſs in its metallic form, WENZEL 42.; which ſhews that the affinity of calx of mercury to phlogiſton is increaſed by heat. The difference betwixt the quieſcent and divellent powers is indeed very ſmall.

Silver appeared to me to precipitate mercury from the nitrous acid, though very ſlowly, when the ſolution of mercury was made with heat, and not over ſaturated; but when the ſolution of mercury was made without heat, it was not at all precipitated. On the other hand, mercury precipitates ſilver from this acid, not by virtue of the ſuperiority of the uſual divellent powers, but by reaſon of the attraction of mercury and ſilver to each other, for they form partly an amalgama and partly vegetate, and ſcarce any of either remains in the ſolution. The ſame thing happens, that is, they vegetate, if ſolutions of both metals in the ſame acid be mixed together.

Silver does not precipitate mercury from the ſolution of ſublimate corroſive; but, on the contrary, mercury precipitates ſilver from the marine acid: and if a ſolution of horn ſilver in volatile alkali be triturated with mercury, the ſilver will

will be freed from its acid and calomel formed, 1 MARGRAAF 284.; and yet, if calomel and ſilver be diſtilled, the mercury will paſs in its metallic form, and horn ſilver will be formed, *ibid.* 2 6. The ſame thing happens if ſilver and ſublimate corroſive be diſtilled, 1 POTT. 338. STAHL *des ſels*, 306; the affinity of calx of mercury to phlogiſton increaſing with the heat.

Of precipitations of and by biſmuth.

With reſpect to the vitriolic acid I have made the ſum of the quieſcent and divellent powers equal, though in fact ſometimes the one preponderates and ſometimes the other. Wiſmuth precipitates nothing from vitriol of copper in 16 hours; nor does copper from vitriol of wiſmuth. Copper is ſaid to precipitate wiſmuth from the nitrous acid; but I have alſo ſeen copper precipitated from this in its metallic form by wiſmuth. The variations proceed from the different dephlogiſtication of copper.

Of precipitations of and by nickel.

Unleſs nickel be pulveriſed it ſcarcely precipitates any metal.

Zinc precipitates a black powder from the ſolution of nickel in the vitriolic and nitrous acid, which Mr. BERGMAN, by a method peculiar to him, has ſhewn to conſiſt of arſenic, nickel, and a little of the zinc itſelf. The arſenic attracting the calx of nickel *; but zinc precipitates nickel from the marine acid.

The ſolution of iron in vitriolic acid acts on nickel, and that of nickel in this ſame acid acts on iron; but neither precipitates the other in 24 hours; but on longer reſt, iron ſeems to have the advantage; but iron clearly precipitates nickel

* SVENSK. Handling. 1780.

from the nitrous acid; and though nickel ſeems alſo to precipitate iron, yet this ariſes only from the gradual dephlogiſtication of the iron.

Nickel precipitates copper in its metallic form from the vitriolic acid. It alſo precipitates copper from the nitrous and marine acids; but copper precipitates arſenic from a nitrous ſolution of nickel. The vitriolic and nitrous ſolutions of lead ſeem to act in ſpecie on nickel, that is, to diſſolve it without any decompoſition, the calces uniting to each other. The vitriolic and nitrous ſolutions of nickel for ſome time act on lead in the ſame manner; but at laſt nickel ſeems to have the advantage. With regard to the marine acid, lead ſeems to have the advantage, though a black precipitate is ſeen, whichever of them is put into the ſolution of the other.

Nickel readily precipitates wiſmuth from the vitriolic and nitrous acids; but as to the marine I found each of theſe ſemimetals ſoluble in the ſolution of the other, yet nickel precipitates wiſmuth very ſlowly, and only as to part; and wiſmuth precipitates a red powder, which I take to be ochre, from the ſolution of nickel.

Nickel and tin are ſlightly acted on, each by the ſalt of the other; but the precipitations are as indicated by the balance of affinities.

Of precipitations of and by cobalt.

Cobalt is not precipitated either from the vitriolic or nitrous acid by zinc; but it ſeemed to me to be precipitated by zinc from the marine acid.

Though iron precipitates cobalt from the three acids, yet I found much of the cobalt retained both by the vitriolic and nitrous acids, particularly the latter, which, after letting fall

the cobalt, afterwards re-takes it, and lets fall the dephlogiſticated calx of iron.

Nickel alſo, though it does not precipitate cobalt itſelf, as appears by the remaining redneſs of the ſolution, yet conſtantly precipitates ſome other heterogenous ſubſtance from it. The ſolution of cobalt in the marine acid becomes colourleſs by the addition of nickel.

Wiſmuth is ſoluble in the vitriolic and nitrous ſolutions of cobalt, and cauſes a ſmall white precipitate, but does not affect the true cobaltic part. Theſe ſolutions in vitriolic acid cannot be attributed to an exceſs of acid, as they are made in a dilute acid, and without heat. Copper alſo precipitates a white ſubſtance from the nitrous ſolution of cobalt, which I take to be arſenic.

It is difficult to procure either nickel or cobalt very pure; it is evident thoſe I uſed were not ſo.

Of precipitations of and by regulus of antimony.

Copper neither precipitates, nor is precipitated from, the vitriolic acid by regulus of antimony, at leaſt in three days; but vitriol of antimony in ſpecie diſſolves it ſlowly.

The regulus is alſo acted upon by vitriol of lead, for it becomes red after remaining 16 hours in the ſolution of that vitriol; and lead ſcarcely precipitates it from the vitriolic acid.

I alſo found, that powdered regulus precipitates vitriol of mercury very ſlightly.

Wiſmuth neither precipitates, nor is precipitated by, this regulus from the vitriolic acid in 24 hours.

Though tin precipitates this regulus from the nitrous acid, yet if the regulus be put into a ſolution of tin in this acid, in 16 hours neither will be found in the ſolution, either

by

by reaſon of the dephlogiſtication, or of the union of the calces to each other.

Iron does not precipitate this regulus intirely from the marine acid, but a triple ſalt ſeems to be formed, conſiſting of the acid and both calces. The regulus is alſo ſoluble in marine ſalt of iron.

Neither does copper precipitate the regulus from marine acid in 16 hours; and if the regulus be put into marine ſalt of copper it will be diſſolved, and volatile alkalies will not give a blue but a yellowiſh white precipitate, ſo that here alſo a triple ſalt is formed.

Of Precipitations of and by regulus of arſenic.

The ſolutions of arſenic act in moſt caſes like two acids: thus iron, copper, lead, nickel, and zinc, are acted on by vitriol of arſenic (that is, its ſolution in vitriolic acid) but ſcarce give any precipitate.

Neither does iron precipitate arſenic from the nitrous acid, but copper does, and even ſilver gives a ſlight white precipitate; but regulus of arſenic precipitates ſilver completely in 16 hours. Hence the former precipitate ſeems to be a triple ſalt.

Mercury alſo ſlightly precipitates arſenic from the nitrous acid, and ſeems to unite to it, yet is itſelf precipitated by regulus of arſenic in 24 hours.

Wiſmuth forms a ſlight precipitate in the nitrous ſolution of arſenic; but regulus of arſenic forms a copious precipitate in the nitrous ſolution of wiſmuth; ſo that I believe the calces unite.

Nickel does not precipitate arſenic from the nitrous acid, but both calces unite; but regulus of arſenic produces a copious precipitate in the nitrous ſolution of nickel, yet the liquor continues

green; ſo that certainly the nickel is not precipitated; the white precipitate in this caſe ſeems to be ſlightly dephlogiſticated arſenic.

This regulus alſo cauſes a white precipitate in the nitrous ſolution of cobalt, but the liquor ſtill continues red.

With regard to the marine acid, copper precipitates the regulus, but volatile alkalies do not ſtrike a blue with this ſolution, which ſhews the copper unites with the arſenic. Iron alſo precipitates the arſenic. Tin is ſoluble in marine ſolution of arſenic, but I could obſerve no precipitate, nor does regulus of arſenic precipitate tin.

Neither wiſmuth nor the regulus of arſenic precipitate each other from the marine acid in 16 hours. Regulus of antimony is alſo acted upon by the marine ſolution of arſenic, though it cauſes no precipitate, nor does the regulus of arſenic precipitate it.

IV. *A Deſcription of a Species of Sarcocele of a moſt aſtoniſhing Size in a Black Man in the Iſland of* Senegal; *with ſome Account of its being an endemial Diſeaſe in the Country of* Galam. *By* J. P. Schotte, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 19, 1782.

THERE are certain diſeaſes which are peculiar to certain countries only, and are thence called endemial ones of ſuch particular countries where they occur. The more progreſs we make in the diſcoveries of countries, the more we are convinced of this fact, and the greater is the number of thoſe diſeaſes that become known. Their formation may depend on climate, food, water, hereditary diſpoſition, and other cauſes. Many endemial diſeaſes of the moſt diſtant countries have been deſcribed by ingenious travellers; but as the Europeans have not yet penetrated into the interior parts of many countries, it is probable, that there may be ſeveral more of this kind, entirely unknown to us. A diſeaſe of this claſs, which I have ſeen at Senegal, and which, as far as I know, has not yet been mentioned by any author, convinces me of what I have advanced; and as it is a remarkable one, I think a ſhort deſcription of it may not be unacceptable to the curious in phyſic.

Mr. BISHOPP, ſurgeon in chief of the province of Senegambia (who now reſides in London) telling me one day, that he was going to ſee a poor black man of the Bambara nation, afflicted with a

moſt extraordinary and dreadful diſeaſe in his teſticles, I accompanied him, being glad of the opportunity of ſeeing it. We entered the hut, and ſaw the man lying on a negro bed, elevated about a foot from the ground. He ſaid to Mr. BISHOPP, that there was again an ulcer on his ſcrotum, which had made him take the liberty to requeſt his attendance. I looked at the ſcrotum, and found it of an aſtoniſhing ſize; but the place where he lay being dark, the hut having no windows, and thoſe people having no candles, he was aſked, if he could not walk towards the door, that we might ſee better. He anſwered, that he would try; but this was attended with much difficulty. A long cotton ſheet was firſt ſpread on the ground before the bed, which being done, he took, with both his hands, the enormous ſcrotum, moved it gradually on the border of the bed, let it ſlide down gently, and put it into the middle of the ſheet: after this he took the two ends of the ſheet, paſſed them up the fore-part of his body, over his ſhoulders, and had them tied behind his neck. This being done he got up, placing the right-hand upon his right-thigh, and holding the ſheet with the left-hand, and proceeded in this manner, with his knees a little bent, ſlowly towards the door, partly ſliding the ſcrotum on the ground, and partly ſupporting it with his neck by means of the ſheet. I was aſtoniſhed at its enormous ſize, when I ſaw it in the light, and yet I neglected to meaſure it, thinking at the time, as is often the caſe, that I ſhould have opportunities enough to do it; but the ſudden invaſion of the iſland by the French prevented me afterwards from performing it. However, according to my gueſs, and without any exaggeration, the whole maſs might be about two feet and a half long from the os pubis to its lower extremity, and about eighteen inches in diameter acroſs from thigh to thigh.

Its

Its weight I will only ſtate at fifty pounds, as it was eſtimated by Mr. BISHOPP, though I believe it to have been more, and indeed from its dimenſions, and from its being a ſolid maſs, it muſt certainly have exceeded that weight. It was of an oblong form, and reſembled in ſome meaſure the ſhape of the ſcrotum of a bull. It felt very hard to the touch, and the ſkin of it was ſo tight, that it could not be pinched by the fingers The penis was quite hid in the bulk, as generally happens when the ſcrotum is much extended, and may be eaſily comprehended by thoſe who have ſeen large ruptures. The ſkin of the perinæum and of the abdomen was drawn downwards, the navel being nearer to the os pubis than it is in the natural ſtate. There was a large aperture formed by the ſkin about a foot downwards from the os pubis, rather inclining towards the right-ſide, out of which the urine came, which, however, did not run in a ſtream, but came irregularly from all the interior ſides of the aperture. When he made water, he inclined the maſs, which reſted on the ground, a little forwards, and he held a wooden bowl cloſe underneath the aperture, into which the urine was immediately received, that it might not run along the maſs, and occaſion excoriation.

There was an ulcer on the anterior part of the ſcrotum, rather towards the left-ſide, of about two inches long, and one inch broad and deep. He ſaid, that it had begun with a puſtule or boil, which being broke had gradually increaſed to this extent. The pus which came from it was white, thick, and of a good kind. The bottom of it was red, and, when touched with the probe, gave him very acute pain. The edges of it were not very callous, and in appearance it did not much differ from an ulcer of a good kind in any other fleſhy part of the body. No other remedies were applied to it but thoſe generally

uſed in common ulcers. It was filled up from the bottom with lint; a pledget of baſilicum was put over it, and the edges were now and then touched with blue vitriol. By thoſe means granulations began to ſhoot from all ſides, the ſore filled up gradually, and a cicatrix was formed. He had had ſmaller ulcers of this kind in other parts of the ſcrotum before this time, which, Mr. BISHOPP told me, he had treated with the ſame ſucceſs.

The man was rather thin than fat, and might be about fifty years old. He himſelf, like moſt blacks, did not know his age; and if he had pretended to know it, I might, perhaps, not have believed him: for as old age is much reſpected among thoſe people, they are very apt, when they are once paſſed fifty, and have grey hair, to call themſelves older than they really are, in order to command reſpect. His abdomen ſeemed rather empty, and appeared drawn in towards the ſpine; yet I do not think, that any of the inteſtines had deſcended into the ſcrotum, or if any had paſſed down, the annuli of the abdomen muſt have been ſo dilated as not to occaſion the leaſt obſtruction in them; for he never had, to my knowledge, any of thoſe complaints or ſymptoms which attend ruptures. Beſides this, it is to be obſerved, that ruptures are not very common among the blacks about Senegal; indeed I can ſay, that I never ſaw one of them.

Having thus far given an account of what I ſaw myſelf of this remarkable diſeaſe, I ſhall now relate what I have been credibly informed of by other people concerning its beginning and progreſs. The man had been purchaſed up the river as a ſlave, when he was about the age of puberty, and brought down to Senegal, where he was kept as a houſe-ſervant by an opulent inhabitant. He was for ſome years healthy and well; but afterwards

afterwards his teſticles began to ſwell inſenſibly, without inflammation, pain, or any other inconvenience. They increaſed gradually, though ſlowly, and became ſome years after of ſuch a bulk, that he was neither able to walk nor perform his uſual work. That he might, however, not be quite idle, as he was otherwiſe a ſtout and able fellow, he uſed to cut bars of iron into pieces of a foot long, which bear a certain price at Senegal, and go among the blacks like current money. This he could do ſitting with a chiſel and hammer, and a ſmall anvil placed before him on the ground, his legs bent under him, and the big ſcrotum reſting on the ground. Mr. BISHOPP had ſeen him perform this work for many years; at laſt, however, the ſcrotum increaſed to ſuch a degree, that the great bulk prevented him from doing it any longer. From the time that the diſorder had firſt begun to ſhew itſelf to the time I ſaw him, five and twenty years had elapſed; he was alive when I left the iſland in February, 1779, and may be ſo now.

This man was the one I ever ſaw afflicted with this diſeaſe at Senegal; but I am credibly informed, that it is endemial in a country which goes, among the blacks at Senegal, by the general name of Galam, and of which this man was a native. This country lies eaſt of Senegal, at the diſtance of about nine hundred Engliſh miles, and its inhabitants are called Bambaras. I have been told by thoſe inhabitants of Senegal, who go annually in the rainy ſeaſon in a fleet of ſmall craft to Galam for trade, that this diſeaſe is particularly common among the chiefs or noblemen of that country, who are ſtyled in their own country language Batcherees; and that they have large wooden bowls, fixed on the fore-part of the ſaddle, into which they place the big ſcrotum when they take a ride on horſe-back. Though this latter circumſtance ſeem a little romantic, yet as

it has been related to me, not by one but by many, ſeparately and at different times, I give it credit, and I have not the leaſt heſitation to believe, that the diſeaſe is common there. Many of the inhabitants of Senegal have applied to me previouſly to their ſetting off for that country, and aſked me, if I could not give them medicines which would cure that diſorder, with a promiſe, that if they proved ſucceſsful, I might be ſure of a very ample reward of gold; but the improbability of ſucceeding in the reduction of ſuch enormous maſſes to their priſtine ſtate prevented me from giving them any.

When I was at Fort James in the river Gambia, for a ſhort time in the year 1776, I was told by ſome Marahbuts, or Mahometan prieſts, of the Mandinga nation, that this diſeaſe was now and then to be met with among the chiefs of their nation, and that they knew no cure for it*. I have no reaſon to diſcredit

* It is to be obſerved, that thoſe Marahbuts apply themſelves, beſides religious matters, to the ſtudy of phyſic; but only as far as it reſts on experience alone, without entering into the inveſtigation of the cauſes of diſeaſes. They are alſo often called upon by the kings and chiefs to give their opinion in points of law and equity. Moſt of them are well verſed in the Arabic language of the Mauritanic dialect, and they are the only people of letters among the blacks; for none of the black nations about Senegal and Gambia have even an alphabet, much leſs any writings in their own languages. I believe the ſelling of charms conſtitutes the greateſt part of their revenue: and the more reputation one of them has acquired, the dearer he ſells them. Thoſe charms uſually conſiſt in nothing but a few lines taken from the Koran, written on a little piece of paper, which, after being ſewed up very nicely in leather or cloth, the buyers wear about their bodies. They are to defend and protect them in dangers; but, as one charm has only the power of protecting them againſt one ſingle kind of danger, they are obliged to have a great many of them, in order to have a protection againſt every probable danger that may befal them; hence many of the blacks are covered with them in different parts of the body; and they have ſuch a ſtrong faith in them,

diſcredit their aſſertion, and what makes it more probable to me is, that the Mandinga and Bambara nations ſeem to be nearly related to one another in outward appearance, cuſtoms,

them, that when they are ſurprized in the night-time by an enemy, they will not take up arms for their own defence, though in the moſt imminent danger, till they have dreſſed themſelves with thoſe charms, and then they will meet him undauntedly. This faith in charms, however, is a corruption of the Mahometan religion, and the Moors, who live on the north-ſide of the river Senegal, obſerving it in its purity, make no uſe of them. The Marahbuts of the black nations, as well as thoſe of the Moors, are alſo the principal merchants and the moſt opulent people among them, and the gum trade on the river Senegal is chiefly carried on by thoſe of the Moors. The Marahbuts are alſo the only people who can travel with any ſafety into diſtant kingdoms, which no layman can well do without running the riſk of being made a ſlave. Their religious profeſſion protects them every where; they are even reſpected among thoſe nations who are not Mahometans; and they are conſidered by them as godly and virtuous people, and men of wiſdom. They make proſelytes in the Mahometan religion every where; and I am inclined to believe, that they will extend and ſpread it in time all over Africa. I have ſeen ſome Marahbuts of the Pool or Fool nation at Senegal who were pretty well verſed in the old teſtament, and knew partly the hiſtory of the inſtitutor of the new one. One day as I was talking with them on the writings of Moſes, happening not rightly to recollect the lineage from Adam to Abraham, one of them flattened the ſand, made it even, and drew with his fingers on it the genealogy from Adam down to Jacob, which, to the beſt of my recollection, correſponded with that given by Moſes. While he was doing this, I looked at him with pleaſure and ſatisfaction, becauſe it reſembled ſo much the rude ſimplicity of the early ages. The Marahbuts reaſon in general exceedingly well on ſuch ſubjects as they are acquainted with, but they have a way, like the eaſtern nations, of adducing parables or ſimiles in their arguments which do not always bear the ſtricteſt reſemblance to the caſe in hand, though they are very perſuaſive with ſuch people as are not capable of inveſtigating the points in which they differ from the caſe in queſtion. I was always much delighted with their converſation, and was often ſorry that I was not maſter of their different languages, and able to converſe with them without an interpreter. The Marahbuts of the Moors are more learned and ingenious in every reſpect than thoſe of the black nations; but I had not much opportunity of converſing with them, as they were not allowed to reſide on the iſland.

and language, though not entirely in religious matters; ſo many of the Mandingas are Mahometans, which the Bambaras are not. Their languages reſemble one another ſo nearly that a Bambara from Galam, and a Mandinga from the kingdom of Barrah, which extends from the ſea-coaſt along the north-ſide of part of the river Gambia, can partly underſtand one another. Both nations have alſo a cuſtom of marking their children in various manners by inciſions in the ſkin, and that of filing their fore teeth (incifores) till they become quite pointed, which I imagine they conſider as being handſome.

As the diſeaſe, according to the information I received, begins with a gradual ſwelling of the teſticles without any pain or inflammation, I am inclined to conſider it as a ſarcocele. HEISTER, in his Surgical Inſtitutions, ſays, that the diſeaſe begins and increaſes moſtly in the ſame manner, when it affects the teſticles themſelves; but that he never ſaw any of them much bigger than a man's fiſt. This difference in the ſize does, in my opinion not, alter the diſeaſe; for we know, that the Bronchocele is hardly known in ſome countries, that it is of a moderate ſize in ſome others, and that in others again it has been ſeen to increaſe to ſuch an enormous bulk as to hang down over the breaſt and belly; yet this difference of ſize does not alter the nature of the diſeaſe, and it ſtill retains the ſame name.

It is difficult to point out the cauſes of ſuch a ſarcocele, as conſiſts in the ſpontaneous tumefaction of the teſticles themſelves; neither do I find any ſatisfactory ones aſſigned by the author I have juſt now quoted; and as I have not been in Galam, I can hardly ſay any thing probable concerning thoſe of the diſeaſe I have deſcribed, I ſhall, however, ſuggeſt the following.

As polygamy is lawful and cuſtomary among the Bambaras as well as among all the other nations about the river Senegal and Gambia, and as the riches and conſequence of a man are eſtimated by the number of wives that he keeps, the chiefs of the people have always a great number of them. I have been told, that the Batcherees of Galam have their victuals moſt immoderately ſeaſoned with Cayenne pepper; and I know myſelf, that the opulent people of the Mandinga nation make the ſame abuſe of it. This may, perhaps, be done with a view to its operating as a provocative; for it has a peculiar effect on the ſeminal veſſels, and will produce erections, attended with a dull pain and turgeſcency in the teſticles: I was therefore inclined to think, that the immoderate uſe of this pepper might partly be the cauſe of this diſeaſe; but then again this could not be the caſe in the man I ſaw at Senegal, where none, or at leaſt very little of it, is uſed.

The moſt probable cauſe of it ſeems to be an hereditary diſpoſition; for, as it only begins to ſhew itſelf about the age of twenty-five or thirty, a man may be father of a great many children before it takes place, and as it ſeems to be confined to families of the principal people of the Bambara nation, it may be, that the man I ſaw afflicted with it at Senegal was deſcended from ſuch a family, and made a ſlave in his younger years by ſome fatal accident or other, as is often the caſe in thoſe countries.

The French, who are the preſent poſſeſſors of the river Senegal, may perhaps be able to give ſhortly a more perfect account of this remarkable endemial diſeaſe. In the mean time, if this ſhould be deemed worthy the notice of the Royal Society, it will afford the greateſt ſatisfaction to him, who has the honour to be, &c.

V. *A Description of a new Construction of Eye-glasses for such Telescopes as may be applied to Mathematical Instruments.* By *Mr.* Ramsden; *communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read December 19, 1782.

TO correct the errors in eye-glasses, arising from their spherical figure, and also from the different refrangibility of light, it has been held absolutely necessary to have two, placed in such manner, that the image formed by the object-glass of the telescope should be between them; but in those telescopes that are applied to mathematical instruments, the interference of the first eye-glass before the image is formed is productive of many bad consequences; should that eye-glass have the least shake or motion whatever, it totally alters the adjustment of the instrument; and the diminishing also of the image by this position, obliging us to shorten the focus of the nearer eye-glass, the wires in the focus of the telescope are thereby considerably more magnified than they would have been with the same power, had both the eye-glasses been put between the image and the eye.

Many defects in the micrometer with moveable wires are caused by the construction of the eye glasses of the telescope to which it is applied. If only one eye-glass is used, the field is so contracted, that it is impossible to measure the diameter of the sun or moon with precision, if the telescope magnifies above 30 times;

times; and if, to enlarge the field, we uſe the preſent conſtruction of two eye-glaſſes, the conſequence is yet worſe; becauſe equal ſpaces between the wires will not then correſpond to equal ſpaces on the objects it repreſents, as thoſe converſant in the theory of optics well know; and this inequality depending on the form, poſition, and refractive power of the firſt eye-glaſs, it will be impoſſible to have data ſufficiently exact to allow for that error.

Thoſe who were ſenſible of this defect have thought to correct it by the application of an achromatic eye-glaſs, on the principle of that kind of object-glaſs, not ſuppoſing it poſſible to correct the aberrations from the different refrangibility of light, and alſo from the ſpherical figure of the lenſes by any other means than combining a concave lens with the convex ones; but the violent and contrary refractions from the neceſſary large ſize of the lenſes in proportion to their focal lengths, not only occaſioned great loſs of light, but rendered it impoſſible to correct the ſpherical aberration ſo as to obtain an angle of viſion much larger than could be had by a ſingle eye-glaſs: yet, however abſurd it may have appeared to attempt correcting both aberrations, when the lenſes are both convex, and are on the near ſide of the wires, the following obſervations will ſhew the practicability of it, and may throw ſome light on the theory of eye-glaſſes which ſeems hitherto not well underſtood.

Sir ISAAC NEWTON has ſhewn in his *Lectiones Opticæ*, in that ſection *De Phænomenis lucis per priſma in Oculum tranſmiſſæ*, that the appearance of colours on the edges of objects when viewed through a priſm depends on the proportion of the diſtance between the priſm and the object, compared with that between the priſm and the eye, that is to ſay, the nearer the object is

brought

brought to the priſm, the leſs will be the bordre of colours on the contours of the object.

To apply this to practice I placed a plano convex lens *a* (*vide* fig. 1.) with its plane ſide near an object, or an image IN formed by the object-glaſs of a teleſcope, and thus magnified the image which, from the poſition of the lens, was ſenſibly free from colour; but the reſpective foci of a lens ſo placed being very near each other, and on the ſame ſide, the emergent pencils diverge on the eye, and give indiſtinct viſion: this was remedied by placing a ſecond lens *k* a little within the focus of the former, the combined foci of the two lenſes being in the place of the image, the rays were thereby made to fall parallel on the eye, and to ſhew the object IN diſtinctly. If, by putting the lens *a* very near the image, any imperfection in it becomes too viſible, that diſtance may be conſiderably increaſed, without producing any bad effect; for theory, as well as experiment, ſhews, that a ſmall aberration from the different refrangibility of light is of little conſequence compared with the ſame quantity of aberration cauſed by the ſpherical figure of the lenſes, but even that colouring may be corrected in the nearer eye-glaſs: for let a ray (fig. 2.) from an object *o*, by paſſing through a lens B, be ſeparated into colours, *ac* being the direction of the violet rays, and *a t* that of the red; if another lens be put at *c*, the violet rays paſſing through its center will ſuffer no refraction, while thoſe of the red, paſſing at ſome diſtance from thence are refracted, and the emergent red and violet will be parallel, when the mean refracting angle of the lenſes at the incidence of each pencil are to one another inverſely as the diameters of thoſe pencils.

If we attend to this poſition of the eye-glaſſes, it will be found equally advantageous for obviating the ſpherical aberration

of

of an oblique pencil as that from colour. In both, where there is a neceſſity for having a large portion of a ſphere, we have only to make the pencil on ſuch lens as ſmall as poſſible, and we may regulate the direction of the rays in each pencil at pleaſure when they approach the axis of the teleſcope.

To illuſtrate this, let us compare the effect of the ſpherical aberration of a lens on an oblique pencil in this poſition with that produced by the ſame lens, placed as uſual at its focal diſtance from the image. Let AC, fig. 3. repreſent the ſemi-object-glaſs of a teleſcope, CT its axis, E an eye-glaſs, and F the common focus of both the object-glaſs and eye-glaſs. Let AFC be an oblique pencil of homogeneous rays, G and H the points where the axis and the extreme rays paſs through the eye glaſs: the aberration of this pencil from the ſpherical figure of the lens E will be $EG^3 - EH^3$; but as the lens approaches towards F, EG and GH, becoming equal, this cauſe of aberration vaniſhes accordingly. The effects of the lens *k* will be altogether inſenſible from the ſmallneſs of its aperture; or it might be corrected in the figure of the object-glaſs, by making its aberration negative as much as this is affirmative.

It has been uſual to conſider that form and poſition of the eye-glaſſes beſt that would make the pencils from every part of the field interſect each other in the axis of the teleſcope at the place of the eye; but this will be found of little conſequence, ſeeing the diameter of a pencil here is generally much leſs than the pupil, nothing more is requiſite than that the eye may take in the pencils from the different parts of the field at the ſame time: but the field of a teleſcope will be moſt perfect when the conſtruction of the eye-glaſſes is ſuch, that the focus of an extreme and of central pencil are at the ſame diſtance

from the eye. The diſpoſition above deſcribed will be found conformable to that idea.

Let AB (fig. 4.) repreſent an image formed by the object-glaſs of a teleſcope, V the firſt eye-glaſs, as already deſcribed, with its plane ſide towards the image; let AC be the axis of a pencil of rays incident on the firſt ſurface of the lens V, and A*e* an extreme ray of the ſame pencil. Take CF to CA as the ſine of incidence out of air into glaſs is to the ſine of refraction, and F will be the focus of this pencil after paſſing through the firſt ſurface of the lens V. From the point F draw the angle CF*e*, the incident pencil on the ſecond ſurface of this lens, continue the lines FC and F*e* to *b* and *r* reſpectively, and draw the perpendiculars OI and OK on the point C, deſcribe the arc *nd*, and making *cd* to *ab*, as the ſine of refraction out of glaſs into air is to the ſine of incidence, draw *cd* continued till it cuts the axis in P. In like manner, on a center *e* deſcribe the arc *mg*, and making *yg* to *or* as the ſine of the angle of refraction is to that of incidence, draw the line *eg*Q; continue it and the line C*d* backward till they meet each other in *h*, and it will be the focus of the emergent pencil from the ſecond ſurface of the lens V. On the axis CF ſet off the diſtance C*ds* equal to C*h*, and draw *es* and C*e*. Now it is evident from the figure, that the focus of the emergent pencil will be nearer to C than the object itſelf, in the proportion the angle C*se* exceeds the angle CA*e*.

Thus, from the great angle of incidence of the oblique pencil on the ſecond ſurface of the lens, the focus of the emergent pencil is brought nearer to P the ſecond eye-glaſs, while that of the principal pencil remains the ſame, or very nearly ſo; and the image will become more diſtinct towards the edge of the field the nearer P*h* and PT approach to equality.

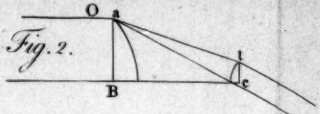


Fig. 2.

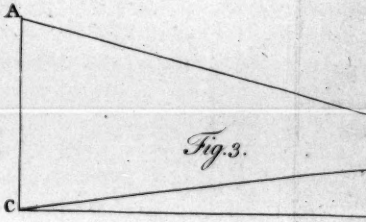


Fig. 3.

Fig.

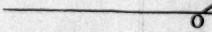
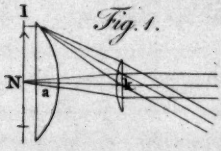


Fig. 1.



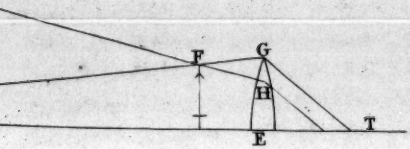
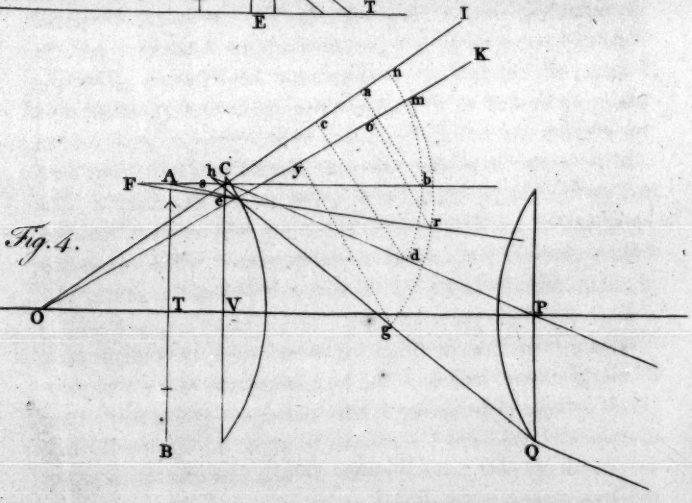


Fig. 4.



To give a proper demonſtration and theorem for the exact form of the firſt lens, according to its diſtance from the image, would require more leiſure than is conſiſtent with the ſituation of one not very converſant with mathematics. That diſtance, in proportion to the focal length of the lens, ſo that any unavoidable defect in it may become inviſible, muſt be determined by experiment. If any variation be made in the form of this lens, it will be better to make the plane ſide rather a little convex than concave. By the latter the image would be diſtorted by the too great obliquity of the rays near the extremity of the lens.

Thus we have a ſyſtem of eye-glaſſes which may be taken out of the teleſcope, in order to wipe them at pleaſure. Or the magnifying power of the teleſcope may be varied without affecting the line of collimation, or in any manner altering the adjuſtment of the inſtrument to which ſuch teleſcopes may be applied with many other advantages. In the preſent improved ſtate of teleſcopes too, the diſagreeable appearance of the wires from the great power of the eye-glaſſes is in a great degree remedied. The ſame principle may be uſefully employed in many other caſes. What is herein contained is only to be conſidered as an explanation of this very uſeful conſtruction, and which is given in hopes that ſome perſon of more abilities in the ſcience of optics will favour us with a general theorem, in order that its application may be more univerſal.

VI. *Account of ſeveral Lunar Iris.* *By* Marmaduke Tunſtall, *Eſq. F. R. S. in Two Letters to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 30, 1782, and January 23, 1783.

TO SIR JOSEPH BANKS, BART. P. R. S.

DEAR SIR,

AS I am ever happy to ſeize on any opportunity to expreſs my regard to yourſelf, and my attention to the Royal Society, I cannot omit this occaſion of acquainting you of rather an unuſual phenomenon ſeen here on Friday night laſt, the 27th of February, between ſeven and eight, eſpecially as it might probably be viſible only at a ſmall diſtance. It was an *Iris lunaris*, or *Lunar rainbow*, in tolerable diſtinct colours, ſimilar to a ſolar one, but more faint; the orange colour ſeemed to predominate. I was unfortunately not a ſpectator myſelf; but can ſufficiently rely on the authority, as a clergyman in my houſe, and ſome ſervants, on whom I can depend, obſerved it for near a quarter of an hour. It happened at full moon, at which time alone they are ſaid to have been always ſeen. Though ARISTOTLE is ſaid to have obſerved two, and ſome others have been ſeen by SUELLIUS, &c. I can only find two deſcribed with any accuracy; *viz.* one by PLOT, in his Hiſtory of Oxfordſhire, ſeen by him in 1675, though without colours; the other ſeen by a Derbyſhire gentleman at Glapwell

near Cheſterfield, deſcribed by THORESBY, and inſerted in N° 331. of the Philoſophical Tranſactions: this was about Chriſtmas, 1710, and ſaid to have had all the colours of the *Iris ſolaris*. The night was windy, and though there was then a drizzling rain and dark cloud, in which the rainbow was reflected: it proved afterwards a light froſt. That this very imperfect account, though it may be ſtrictly relied on, may give any ſatisfaction to you, or the gentlemen of the Society, would be the occaſion of great pleaſure to

DEAR SIR, your much obliged, &c.

Wycliffe near Greta Bridge, Yorkſhire,
March 1, 1782.

The particular circumſtance, which appeared extraordinary to THORESBY, of the bow being nearly equal in ſize to that of the ſolar one, ſeemed to be verified by this, as the extent appeared nearly of the ſame dimenſions. The wind was at ſouth-weſt.

DEAR SIR, Wycliffe, Oct. 23, 1782.

I TROUBLED you early in ſpring with an account of a rather ſingular phenomenon, ſeen here on the 27th of February, between ſeven and eight o'clock, *viz.* a *lunar* Iris with priſmatic colours. Since that I have ſeen two more here; one July the 30th, about eleven o'clock, which laſted about a quarter of an hour, without colours; the laſt, the cauſe of my

my troubling you with this, was on Friday the 18th inſtant, perhaps the moſt extraordinary one of the kind ever ſeen, and of which I was myſelf a ſpectator for moſt of its duration, as were many in my houſe and neighbourhood. It was firſt viſible about nine o'clock, and continued, though with very different degrees of brilliancy, till paſt two. At firſt, though a ſtrongly marked bow, it was without colours; but afterwards they were very conſpicuous and vivid in the ſame form as in the ſolar, though fainter; the red, green, and purple, were moſt diſtinguiſhable. About twelve it was the moſt ſplendid in appearance; its arc was conſiderably a ſmaller ſegment of a circle than a ſolar; its ſouth-eaſt limb firſt began to fail, and a conſiderable time before its final extinction; the wind was very high, nearly due weſt, moſt part of the time, accompanied with a drizzling rain. It is a ſingular circumſtance, that three of theſe phenomena ſhould have been ſeen in ſo ſhort a time in one place, as they have been eſteemed ever ſince the time of ARISTOTLE, who is ſaid to have been the firſt obſerver of them, and ſaw only two in fifty years, and ſince by PLOT and THORESBY, almoſt the only two Engliſh authors who have ſpoke of them, to be exceeding rare. They ſeem evidently to be occaſioned by a refraction in a cloud or turbid atmoſphere, and in general indications of ſtormy and rainy weather, ſo bad a ſeaſon as the late ſummer having, I believe, ſeldom occurred in England. THORESBY, indeed, ſays, the one he obſerved was ſucceeded by ſeveral days of fine ſerene weather.

One particular, rather ſingular, in the ſecond, *viz.* of July the 30th, was its being ſix days after the full of the moon, and the laſt, though of ſo long a duration, was three days before the full; that of the 27th of February was exactly

exactly at the full, which uſed to be judged the only time they could be ſeen, though in the Encyclopedie there is an account that WEIDLER obſerved one in 1719, in the firſt quarter of the moon, with faint colours, and in very calm weather.

No lunar Iris, I ever heard or read of, laſted near ſo long as that on the 18th inſtant, either with or without colours.

VII. *Account of an Earthquake. By* John Lloyd, *Eſq. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 23, 1783.

TO SIR JOSEPH BANKS, BART. P. R. S.

Wickwer near St. Aſaph,
DEAR SIR, Nov. 16, 1782.

UPON Saturday the 5th of October laſt, between eight and nine o'clock in the evening, a ſhock of an earthquake was felt in ſeveral parts of this principality, by many perſons, though not generally. At Mold, in the county of Flint, it was diſtinctly perceived by a gentleman, at that time in a houſe quite out of the town, and ſeemed attended with a rumbling noiſe, like a carriage going over a pavement, ſo that if his ſituation had not rendered that ſenſation impoſſible, he ſhould have aſcribed it to that; and at the ſame time ſome China cups and ſaucers rattled very much, that were upon a table in the room with him.

At the palace, at Bangor, it was perceived by all the biſhop's family at about thirty-nine minutes paſt eight o'clock, with the ſame kind of rumbling, and a double vibration. Many other perſons in that neighbourhood were ſenſible of it.

In many places in the iſle of Angleſey it was ſtrongly felt; at Bodorgan, the ſeat of OWEN P. MEYRICK, eſq. it was thought by the family that a carriage had driven up to the door. In anſwer to ſome enquiries made, I received the following

lowing account of an ingenious friend of mine, who is concerned in the great copper mine at Paris Mountain, and was at that time within a mile of the mine at his own houſe.

" I perceived the earthquake to begin at Amlwoh at 40′ paſt " eight o'clock at night, on Saturday the 5th of October. The " ſhock was great and alarming. The houſe in which I was " was ſhaken terribly, and underwent ſeveral vibrations for " the continuance of near a quarter of a minute. I thought " it moved from N.E. to S.W. but was not certain. It was " attended with a rumbling noiſe, as loud as thunder, and " like it juſt before it ceaſes. I have made an enquiry at ſeveral " diſtant parts in the iſland to the S.W. about it, to have found " out, if poſſible, at what rate it moved, but in vain."

At the time it was felt in the places I have mentioned, I was at St. Aſaph with ſome other gentlemen, looking over ſome pariſh accounts; but none of us perceived it, though it was perceived by a relation of mine, who was then alone, reading at the diſtance of a mile and a half from us, and in the line between Angleſey and Mold, ſo that I was probably further north than the ſhock reached. I judge every phenomenon of this kind to be intereſting to the ſpeculative obſervers of nature. You may probably be of the ſame opinion; and if you are, and ſhould think this imperfect account of ſufficient conſequence, you will pleaſe to lay it before the Royal Society.

I have the honour to be, &c.

VIII. *An Account of a new Eudiometer.* By *Mr.* Cavendiſh, *F. R. S.*

Read January 16, 1783.

DR. PRIESTLEY's diſcovery of the method of determining the degree of phlogiſtication of air by means of nitrous air, has occaſioned many inſtruments to be contrived for the more certain and commodious performance of this experiment; but that invented by the Abbé FONTANA is by much the moſt accurate of any hitherto publiſhed. There are many ingenious contrivances in his apparatus for obviating the ſmaller errors which this experiment is liable to; but the great improvement conſiſts in this, that as the tube is long and narrow, and the orifice of the funnel not much leſs than the bore of the tube, and the meaſure is made ſo as to deliver its contents very quick, the air riſes ſlowly up the tube in one continued column; ſo that there is time to take the tube off the funnel, and to ſhake it before the airs come quite in contact, by which means the diminution is much greater and much more certain than it would otherwiſe be. For inſtance, if equal meaſures of nitrous and common air are mixed in this manner, the bulk of the mixture will, in general, be about one meaſure; whereas, if the airs are ſuffered to remain in contact about one-fourth of a minute before they are ſhaken, the bulk of the mixture will be hardly leſs than one meaſure and two-tenths, and will be very different according as it is ſuffered to

remain a little more or a little leſs time before it is ſhaken. In like manner, if through any fault in the apparatus, the air riſes in bubbles, as in that caſe it is almoſt impoſſible to ſhake the tube ſoon enough, the diminution is leſs than it ought to be.

Another great advantage in this manner of mixing is, that thereby the mixture receives its full diminution in the ſhort time during which it is ſhaken, and is not ſenſibly altered in bulk after that; whereas, if the airs are ſuffered to remain ſome time in contact before they are ſhaken, they will continue diminiſhing for many hours.

The reaſon of the abovementioned differences ſeems to be, that in the Abbé FONTANA's method the water is ſhaken briſkly up and down in the tube while the airs are mixing, whereby each ſmall portion of the nitrous air muſt be in contact with water, either at the inſtant it mixes with the common air, or at leaſt immediately after; and it ſhould ſeem, that when the airs are in contact with water during the mixing, the diminution is much greater and more certain than when there is no water ready to abſorb the nitrous acid produced by the mixture. This induced me to try whether the diminution would not be ſtill more certain and regular if one of the two kinds of air was added ſlowly to the other in ſmall bubbles, while the veſſel containing the latter was kept continually ſhaking. I was not diſappointed in my expectations, as, I think, this method is really more accurate than the Abbé FONTANA's; and, moreover, in the courſe of my experiments I had occaſion to obſerve a circumſtance which is neceſſary to be attended to by thoſe who would examine the purity of air with exactneſs by any kind of eudiometer, beſides ſome others which tend very

much to explain many of the phenomena attending the mixture of common and nitrous air.

The apparatus I uſe is as follows. A (fig. 1.) is a cylindrical glaſs veſſel, with braſs caps at top and bottom; to the upper cap is fitted a braſs cock B; the bottom cap is open, but is made to fit cloſe into the braſs ſocket D*d*, and is fixed in it in the ſame manner as a bayonet is on a muſquet. The ſocket D*d* has a ſmall hole E in its bottom, and is faſtened to the board of my tub by the bent braſs F*f*G, in ſuch manner that *b*, the top of the cock, is about half an inch under water; conſequently if the veſſel A is placed in its ſocket, with any quantity of air in it, and the cock is then opened, the air will run out by the cock, but will do ſo very ſlowly, as it can eſcape no faſter than the water can enter by the ſmall hole E to ſupply its place.

Beſides this veſſel, I have three glaſs bottles like M (fig. 2.) each with a flat braſs cap at bottom to make it ſtand ſteady, and a ring at top to ſuſpend it by, and alſo ſome meaſures of different ſizes ſuch as B (fig. 3.); theſe are of glaſs with a flat braſs cap at bottom and a wooden handle. In uſing them they are filled with the air wanted to be meaſured, and then ſet upon the braſs knob C fitted upon the board of my tub below the ſurface of the water, which drives out ſome of the air, and leaves only the proper quantity. This meaſure is eaſier made, and more expeditious in uſing, than the Abbé FONTANA's, and, I believe, is equally accurate; but if it was not it would not ſignify, as I determine the exact quantity of air uſed by weight.

There are two different methods of proceeding which I have uſed; the firſt is to add the reſpirable air ſlowly to the nitrous; and the other, to add the nitrous air in the ſame manner to the reſpirable.

reſpirable. The firſt is what I have commonly uſed, and which I ſhall firſt deſcribe. In this method a proper quantity of nitrous air is put into one of the bottles M, by means of one of the meaſures above deſcribed, and a proper quantity of reſpirable is let into the veſſel A, by firſt filling it with this air, and then ſetting it on the knob C, as was done by the meaſure. The veſſel A is then fixed in the ſocket, and the bottle M placed with its mouth over the cock. Then on opening the cock, the air in the veſſel A runs ſlowly in ſmall bubbles into the bottle M, which is kept ſhaking all the time by moving it backwards and forwards horizontally while the mouth ſtill remains over the cock.

Notwithſtanding the precautions uſed by the Abbé FONTANA in meaſuring the quantity of air uſed, I have ſometimes found that method liable to very conſiderable errors, owing to more water ſticking to the ſides of the meaſure and tube at one time than at another: for this reaſon I determine the quantities of air uſed and the diminution, by weighing the veſſels containing it under water in this manner. From one end of a balance, placed ſo as to hang over the tub of water, is ſuſpended a forked wire, to each end of which fork is fixed a fine copper wire; and in trying the experiment the veſſel A, with the reſpirable air in it, is firſt weighed, by ſuſpending it from one of theſe copper wires, in ſuch manner as to remain intirely under water. The bottle M, with the proper quantity of nitrous air in it, is then hung on in the ſame manner to the other wire, and the weight of both together found. The air is then let out of the veſſel A into the bottle M, and the weight of both veſſels together found again, by which the diminution of bulk which they ſuffer on mixing is known. Laſtly, the bottle M is taken off, and the veſſel A weighed

again by itſelf, which gives the quantity of reſpirable air uſed. It is needleſs to determine the quantity of nitrous air by weight; becauſe, as the quantity uſed is always ſufficient to produce the full diminution, a ſmall difference in the quantity makes no ſenſible difference in the diminution *. In this manner of determining the quantities by weight, care ſhould be taken to proportion the lengths of the copper wires in ſuch manner that the ſurface of the water in A and M ſhall be on

* Mr. DE SAUSSURE alſo determines the quantity of air which he uſes by weight; but does it by weighing the veſſels containing it in air. This method is liable to ſome inaccuracy, as the air in the veſſel is apt to be compreſſed by putting in the ſtopper; though, I believe, that, if care is taken to puſh in the ſtopper ſlowly, the error ariſing from thence is but ſmall. It is alſo leſs expeditious than weighing them under water, as ſome time is neceſſarily loſt in wiping the wet off the veſſels; but, on the other hand, it requires leſs apparatus, which makes it fitter for a portable apparatus as Mr. DE SAUSSURE's was. If any gentleman is deſirous of adapting this method of determining the quantities to the above deſcribed manner of mixing the airs, nothing more is required than to have glaſs ſtoppers fitted to the veſſel A and to the bottle M.

It is needleſs to mention, that in both theſe methods no ſenſible error can ariſe from any difference in the ſpecific gravity of the air; for the thing found by weighing the veſſel is the difference of weight of the included air and of an equal bulk of water, which, as no air is leſs than 500 times lighter than water, is very nearly equal to the weight of a quantity of water, equal in bulk to the included air.

It muſt be obſerved, that a common balance is not convenient for weighing the veſſels of air under water, without ſome addition to it; for the lower the veſſel of air ſinks under the water, the more the air is compreſſed, which makes the veſſel heavier, and thereby cauſes that end of the beam to preponderate. This makes it neceſſary either to have the index placed below the beam, as in many aſſay balances; or by ſome other means to remove the center of gravity of the beam ſo much below the center of ſuſpenſion as to make the balance vibrate, notwithſtanding the tendency which the compreſſibility of the air in the veſſels has to prevent it.

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the ſame level when both have the uſual quantity of air in them, as otherwiſe ſome errors will ariſe from the air being more compreſſed in one than in the other. This precaution indeed does not intirely take away the error, as the level of the water in M is not the ſame after the airs are mixed as it was before; but in veſſels of the ſame ſize as mine, the error ariſing from thence can never amount to the 500th part of the whole, which is not worth regarding; and indeed if it were much greater, it would be of very little conſequence, as it would be always the ſame in trying the ſame kind of air.

There are ſeveral contrivances which I uſe, in order to diminiſh the trouble of weighing the veſſels; but I omit them, as the deſcription would take up too much room.

The veſſel A holds 282 grains of water, and is the quantity which I ſhall diſtinguiſh by the name of one meaſure. I have three bottles for mixing the airs in, with a meaſure B for the nitrous air adapted to each. The firſt bottle holds three meaſures, and the correſponding meaſure 1¼; the ſecond bottle holds ſix, and the correſponding meaſure 2½; and the third bottle holds 12, and the correſponding meaſure 5. The firſt bottle and meaſure is uſed in trying common air, or air not better than that; the two other in trying dephlogiſticated air. The quantity of reſpirable air uſed, as was ſaid before, is always the ſame, namely, one meaſure; conſequently, in trying common air I uſe 1¼ meaſures of nitrous air to one of common; and in trying very pure dephlogiſticated air I uſe five meaſures of nitrous air to one of the dephlogiſticated. I believe there is no air ſo much dephlogiſticated as to require a greater proportion of nitrous than that. The way by which I judge whether the quantity of nitrous air uſed is ſufficient, is by the bulk of the two airs when mixed; for if that is not

leſs

leſs than one meaſure, that is, than the reſpirable air alone, it is a ſign that the quantity of nitrous air is ſufficient, or that it is ſufficient to produce the full diminution, unleſs it is very impure.

Though the quantity of reſpirable air uſed will be always nearly the ſame, as being put in by meaſure; yet it will commonly be not exactly ſo, for which reaſon the obſerved diminution will commonly require ſome correction: for example, ſuppoſe that the obſerved diminution was 2.353 meaſures, and that the quantity of reſpirable air was found to be .935 of a meaſure; then the obſerved diminution muſt be increaſed by $\frac{65}{1000}$ of the whole or .035, in order to have the true diminution, or that which would have been produced if the reſpirable air uſed had been exactly one meaſure; conſequently, the true diminution is 2.388.

The method of weighing, deſcribed in p. 109. is that which I uſe in trying air much different in purity from common air; but in trying common air, I uſe a ſhorter method, namely, I do not weigh the veſſel A at all, but only weigh the bottle M with the nitrous air in it; then mix the airs, and again weigh the ſame bottle with the mixture in it, and find the increaſe of weight. This, added to one meaſure, is very nearly the true diminution, whether the quantity of common air uſed was a little more or a little leſs than one meaſure. The reaſon of this is, that as the diminution produced on mixing common and nitrous air is only a little greater than the bulk of the common air, the bulk of the mixture will be very nearly the ſame, whether the bulk of the common air is a little greater or a little leſs than one meaſure: for example, let us firſt ſuppoſe, that the quantity of common air uſed is exactly one meaſure, and that the diminution of bulk on mixing is 1.08 of a meaſure, then

then muſt the increaſe of weight of the bottle M, on adding the common air, be .08 of a meaſure. Let us now ſuppoſe, that the quantity of common air uſed is 1.02 of a meaſure, then will the diminution, on adding the common air, be $1.08 \times \frac{1.02}{1.00}$ or 1.1016 of a meaſure, and conſequently the increaſe of weight of the bottle M will be 1.1016 − 1.02 or .0816 of a meaſure, which is very nearly the ſame as if the common air uſed had been exactly one meaſure.

In the ſecond method of proceeding, or that in which the nitrous air is added to the reſpirable, I uſe always the ſame bottle, namely, that which holds three meaſures, and uſe always one meaſure of reſpirable air; and in trying common air uſe the ſame veſſel A as in the firſt method; but for dephlogiſticated air I uſe one that holds 3½ meaſures.

In trying the experiment I firſt weigh the bottle M without any air in it, and then weigh it again with the reſpirable air in it, which gives the quantity of reſpirable air uſed. I next put the nitrous air into the veſſel A, and weigh that and the bottle M together, and then having mixed the airs, weigh them again, which gives the diminution.

From what has been juſt ſaid, it appears, that in this method of proceeding I uſe a leſs quantity of nitrous air in trying the ſame kind of reſpirable air than in the former; the reaſon of which is, that the ſame quantity of nitrous air goes further in phlogiſticating a given quantity of reſpirable air in this than in the former method, as will be ſhewn further on.

In both theſe methods I expreſs the teſt of the air by the diminution which they ſuffer in mixing; for example, if the diminution on mixing them is two meaſures and $\frac{353}{1000}$, I call its teſt 2.353, and ſo on.

In the first method of proceeding I found, that the diminution was scarce sensibly less when I used one measure of nitrous air than when I used a much greater quantity; so that one measure is sufficient to produce the full diminution. I chuse, however, to use $1\frac{1}{2}$, for fear the nitrous air may be impure; $\frac{7}{8}$ths of a measure of nitrous air produced about $\frac{1}{100}$, and $\frac{1}{4}$ths of a measure about $\frac{7}{90}$ths of the full diminution.

I found also, that there was no sensible difference in the diminution whether the orifice by which the air passed out of the vessel A into the bottle M was only $\frac{1}{135}$th of an inch in diameter, or whether it was $\frac{1}{9}$th of an inch; that is, whether the air escaped in smaller or larger bubbles. The diminution was rather less when the bottle was shook gently than when briskly; but the difference between shaking it very gently and as briskly as I could was not more than $\frac{1}{1000}$dth of a measure. But if it was not shaken at all the diminution was remarkably less, being at first only ,9; in about 3′, indeed, it increased to ,93, and after being shaken for about a minute it increased to ,99; whereas, when the bottle was shaken gently, the diminution was 1,08 at first mixing, and did not increase sensibly after that time. The difference proceeding from the difference of time which the air took up in passing into the bottle was rather greater; namely, in some trials, when it took up 80″ in passing, the diminution was $\frac{3}{1000}$dths greater than when it took up only 22″, and about $\frac{5}{1000}$dths greater than when it took up 45″; in some other trials, however, the difference was less. It appears, therefore, that the difference arising from the difference of time which the air takes up in passing into the bottle is considerable; but, as with the same hole in the plate Dd it will take up always nearly the same time, and as it is easy adjusting the size of the hole, so as to make it take up nearly the time

time we deſire, the error proceeding from thence is but ſmall. The time which it took up in paſſing in my experiments was uſually about 50″.

The difference proceeding from the difference of ſize of the bottle, and the nature of the water made uſe of is greater; for when I uſe the ſmall bottle which holds three meaſures, and fill it with diſtilled water, the uſual diminution in trying common air is 1.08; whereas, if I fill the bottle with water from my tub, the diminution is uſually about .05 leſs. If I uſe the bottle which holds twelve meaſures, filled with diſtilled water, the diminution is about 1.15; and if I uſe the ſame bottle, filled with water from my tub, about 1.08.

The reaſon of this difference is, that water has a power of abſorbing a ſmall quantity of nitrous air; and the more dephlogiſticated the water is, the more of this air it can abſorb. If the water is of ſuch a nature alſo as to froth or form bubbles on letting in the common air, the diminution is remarkably leſs than in other water.

The following table contains the diminution produced in trying common air in the bottle containing three meaſures, with ſeveral different kinds of water, and alſo the diminution which the ſame quantity of nitrous air ſuffered by being only ſhook in the ſame bottle, without the addition of any common air, tried by ſtopping the mouth of the bottle with my finger, and ſhaking it briſkly for one minute, and afterwards for one minute more.

Diminution in trying common air.	Diminution on shaking nitrous air for one minute	Diminution on shaking nitrous air for two minutes.	
1.099	.118	.122	Diſtilled water.
1.049	.083	.088	Water from tub.
1.036	.090	.098	Pump water.
1.062	.090	.099	Diſtilled water, in which a few drops of liver of ſulphur were kept for a few days.
1.045	.052	.056	Diſtilled water impregnated with nitrous air, by keeping it with about $\frac{1}{4}$ of its bulk of nitrous air for two days, and frequently ſhaking it.
.897	.082	.085	Water fouled by oak ſhavings. N. B. it frothed very much.

In general, the diminution was nearly as great with rain water as diſtilled water; but ſometimes I have found rain water froth a good deal, and then the diminution was not much greater than by the water fouled with oak ſhavings.

This difference in the diminution, according to the nature of the water, is a very great inconvenience, and ſeems to be the chief cauſe of uncertainty in trying the purity of air; but it is by no means peculiar to this method, as I have found as great a difference in FONTANA's method, according as I have filled the tube with different waters*. But it ſhews plainly, how little all the experiments which have hitherto been made for determining the variations in the purity of the atmoſphere can be relied on, as I do not know that any one before has been attentive to the nature of the water he has uſed, and the difference proceeding from the difference of waters is much greater than any I have yet found in the purity of air.

* I do not find that it makes much difference in FONTANA's method whether the water is diſpoſed to froth or not; but the advantage which it has in that reſpect over this method is not of much conſequence, as it is eaſy, finding water which will not froth.

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The beſt way I know of obviating this inconvenience is to be careful always to uſe the ſame kind of water: that which I always uſe is diſtilled, as being moſt certain to be always alike. I ſhould have uſed rain water, as being eaſier procured, if it had not been that this water is ſometimes apt to froth, which I have never known diſtilled water do.

As I found that the power with which the diſtilled water I uſed abſorbed nitrous air was greater at ſome times than others, which muſt neceſſarily make an error in the obſervation, I was in hopes that, by obſerving the quantity of nitrous air which the water abſorbed in the ſame manner as in the preceding experiment, together with the heat of the water, as that alſo ſeems to affect the experiment, one might be able to correct the obſerved teſt, and thereby obviate the error which would otherwiſe ariſe from any little difference in the nature of the water employed. With this view I made the following experiment.

I purged ſome diſtilled water of its air by boiling, and kept one part of it for a week in a bottle along with ſome dephlogiſticated air, and ſhook it frequently; the other part was treated in the ſame manner with phlogiſticated air. At the end of this time I found, by a mean of three different trials, that the teſt of common air tried with the firſt of theſe waters was 1.139, the diminution which nitrous air ſuffered by being ſhook 2′ in it in the uſual manner was .285. The teſt of the ſame air tried with the laſt of theſe waters was only 1.054, and the diminution of nitrous air only .090, the heat of the water in the tub and of the diſtilled waters being 45°. I then raiſed both the water of the tub and the diſtilled waters to the heat of 67°, and found that the teſt of the ſame air, tried by the firſt water, was then 1.100, and by the latter 1.044; and that the

the diminution of nitrous air was .235 by the firſt water, and .089 by the latter.

It ſhould ſeem from hence, as if the obſerved teſt ought to be corrected by ſubtracting $\frac{4}{10}$ths of the diminution which nitrous air ſuffers by being ſhaken in the water, and adding .002 for every 3° of heat above 0, as the foregoing trials will agree very well together, if they are corrected by this rule, and better than if corrected by any different rule, as will appear by the following table.

	Heat.	Diminution of nitrous air.	Obſerved teſt.	Correction for Diminution.	Correction for Heat.	Corrected teſt.
Former water	45	.285	1.139	.114	.030	1.055
	67	.235	1.100	.094	.045	1.051
Latter water	45	.090	1.054	.036	.030	1.048
	67	.089	1.044	.036	.045	1.053

Though in all probability this correction will diminiſh the error proceeding from a difference in the nature of the diſtilled water employed, yet I have reaſon to think, that it will by no means entirely take it away; for which reaſon I do not in general make uſe of it. In almoſt all the trials, indeed, in which I have applied the correction, it has come out very nearly the ſame; which ſeems to ſhew, that there was no other difference in the abſorbing power of the diſtilled water I employed, than what proceeded from its difference of heat. The above experiment, however, ſhews plainly, that diſtilled water is capable of a very great difference in this reſpect independent of its heat.

In the ſecond method of proceeding, or that in which the nitrous air is added to the reſpirable, I found nearly the ſame difference

difference in the diminution, according as the bottle was ſhaken briſkly or gently, as in the former method: I found alſo nearly the ſame difference, or perhaps rather leſs, according to the nature of the water employed, only it ſeemed to be of not much conſequence whether the water frothed or not; but there ſeemed to be much leſs difference in the diminution, according to the time which the air took up in paſſing into the bottle. The uſual diminution on trying common air with different quantities of nitrous air, when diſtilled water was employed, was as follows:

Common air.	Nitrous air.	Diminution.
1.	.6	.74
	.8	.88
	1.	.89
	1.5	.90

It appears, therefore, that $\frac{3}{10}$ths of a meaſure of nitrous is ſufficient to produce very nearly the full diminution. I chuſe, however, always to uſe one meaſure. It appears alſo, that the diminution is always much leſs in this method than when the common air is added to the nitrous; as in that method it was before ſaid, that the uſual diminution was 1.08. The reaſon of this is, that when nitrous and common air are mixed together, the nitrous air is robbed of part of its phlogiſton, and is thereby turned into phlogiſticated nitrous acid, and is abſorbed by the water in that ſtate, and beſides that, the common air is phlogiſticated, and thereby diminiſhed: ſo that the whole diminution on mixing is equal to the bulk of nitrous air, which is turned into acid, added to the diminution which the common air ſuffers by being phlogiſticated. Now it appears, that when a ſmall quantity of nitrous air comes in contact with a large

quantity of common air, it is more completely deprived of its phlogiſton, and is abſorbed by the water in a more dephlogiſticated ſtate than when a ſmall quantity of common air comes in contact with a large quantity of nitrous; conſequently, in the ſecond method, where ſmall portions of nitrous air come in contact with a large quantity of common air, the nitrous air is more deprived of its phlogiſton, and therefore a leſs quantity of it is required to phlogiſticate the common air than in the firſt method, where ſmall portions of common air come in contact with a large quantity of nitrous air; ſo that a leſs quantity of the nitrous air is abſorbed in the ſecond method than in the firſt. As to the common air, as it is completely phlogiſticated in both methods, it moſt likely ſuffers an equal diminution in both.

A clear proof that a leſs quantity of nitrous is required to phlogiſticate a given quantity of common air in the ſecond method than in the firſt, is, that if common air is mixed with a quantity of nitrous air not ſufficient to completely phlogiſticate it, the mixture will be more phlogiſticated if the nitrous air is added ſlowly to the common, as in the ſecond method, than if the common air is added to the nitrous; and if the nitrous air is added ſlowly to the common, without being in contact with water, the mixture will be found to be ſtill more phlogiſticated than in the ſecond method, where the two airs are in contact with water at the time of mixing.

The following table contains the reſult of the experiments I have made on this ſubject.

First method.			Second method.			Nitrous air added ſlowly to common without being in contact with water.		
Nitrous air.	Bulk of mixture	Teſt.	Nitrous air.	Bulk of mixture	Teſt.	Nitrous air.	Bulk of mixture	Teſt.
.716	.856	.244	.635	.849	.137			
.474	.915	.513	.430	.867	.352	.294	.836	.337
			.280	.930	.599			

The two-firſt ſets of experiments were not tried with the apparatus above deſcribed, as that held too ſmall a quantity, but with another upon the ſame principle. The laſt ſet was tried by the apparatus repreſented in fig. 4. where A is a bottle containing nitrous air, inverted into the tub of water DE; B is a bottle with a bent glaſs tube C fitted to its mouth. This bottle is filled with common air, without any water, and is firſt ſlightly warmed by the hand; the end of the glaſs tube is then put into the bottle of nitrous air, as in the figure; conſequently, as the bottle B cools, a little nitrous air runs into it, which, by the common air in it, is deprived of its elaſticity, ſo that more nitrous air runs in to ſupply its place. By this means the nitrous air is added ſlowly to the common without coming in contact with water, till the whole of the nitrous air has run out of the bottle A into B; then, indeed, the water runs through the glaſs tube into B, to ſupply the vacancy formed by the diminution of the common air.

It appears from the foregoing table, that a quantity of nitrous air, uſed in the firſt method, does not phlogiſticate common air more than three-fourths of that quantity uſed in the ſecond way does, and not ſo much as half that quantity uſed in the third way: ſo that we may ſafely conclude, that it is this circumſtance of the nitrous air going further in phlogiſti-

caring common air in ſome circumſtances than others, which is the cauſe that the diminution in trying the purity of air by the nitrous teſt is ſo much greater in ſome methods of mixing them than in others.

From what was ſaid in p. 119. it ſhould ſeem as if the ſecond method was more exact than the firſt, as the error proceeding from the air employing more or leſs time in paſſing into the bottle was found to be leſs, and that proceeding from a difference in the water, and from the bottle being ſhaken more or leſs ſtrongly was not greater. I, however, have found, that the trials of the ſame air on the ſame day have commonly differed more when made in this manner than in the firſt; for which reaſon, and becauſe in trying common air the firſt method takes up the leaſt time, I have commonly uſed that.

It ſhould be obſerved, that in trying dephlogiſticated air by the firſt method it is convenient to uſe different bottles, according to the different purity of the air; and the ſame air will appear purer, if tried by a larger bottle than by a ſmaller. For example, if its teſt, tried by the large bottle, comes out 2.54, it will appear not more than 2.44, if tried by the middle bottle; and, in like manner, if its teſt by the middle bottle comes out 1.11, it will appear to be about 1.08, if tried by the leaſt bottle; for this reaſon it is right always to ſet down which bottle it is tried by.

I think I may confidently aſſert, that either of the above methods are conſiderably more accurate than FONTANA's, ſuppoſing the experiment to be made exactly in his manner, that is, determining the quantities by meaſure. But, in order to judge which method of mixing the airs is moſt exact, it was neceſſary to determine the quantities in his method alſo by weight, as otherwiſe it would be uncertain whether my method of mixing

mixing the airs is really better than his, or whether the apparent greater exactneſs proceeds only from the ſuperiority of weighing above meaſuring: for this reaſon I made ſome experiments in which common and nitrous air were mixed in his manner, except that I uſed only one meaſure of each, as Dr. INGEN-HOUSZ did, and that the nitrous air was put up firſt, the true diminution being determined by weight, by firſt weighing the tube under water with the nitrous air in it, and then adding the common air, and weighing the tube again under water. It was unneceeſſary, for the reaſons given in p. 110. and 112. to determine the quantity of either the nitrous or common air by weight. My reaſon for this variation was, that it afforded a much eaſier method of determining the quantities by weight, was leſs trouble, and, I believe, muſt be at leaſt as exact; for I have always found, that the experiments made with the Abbé FONTANA's apparatus, in which I uſed only one meaſure of each air, agreed better together than thoſe in which I uſed two of common, and added the nitrous air by one at a time; and I imagine it can be of no ſignification whether the nitrous or common air is put in firſt, as I cannot perceive the diminution to be ſenſibly greater in one of thoſe ways than the other *.

* It is not extraordinary, that in this method the diminution is juſt the ſame whether the common or nitrous air is put up firſt, notwithſtanding that in mine it is very different; ſince in this method the two airs mix in the ſame manner whichever is put up firſt: whereas in mine, the manner in which they mix is very different in thoſe two caſes; as in one, ſmall portions of common air come in contact with large portions of the nitrous; and in the other, ſmall portions of nitrous air come in contact with large portions of common air.

From the result of these experiments I am persuaded, that my method of mixing the airs is really rather more accurate than FONTANA's, as in trying the same bottle of air six or seven times in my method the different trials would not often differ more than $\frac{1}{100}$dth part, and very seldom more than $\frac{2}{100}$dth; whereas in his there would commonly be a difference of $\frac{1}{100}$dth, and frequently near twice that quantity, though I endeavoured to be as regular as I could in my manner of trying the experiment. My method also certainly requires less dexterity in the operator than his.

It is of much importance towards forming a right judgement of the degree of accuracy to be expected in the nitrous test, to know how much it is affected by a difference in the nitrous air employed. Now it must be observed, that nitrous air may differ in two respects; first, it may vary in purity, that is, in being more or less mixed with phlogisticated or other air; and, secondly, it is possible, that out of two parcels equally pure one may contain more phlogiston than the other. If it differs in the second respect, it will evidently cause an error in the test, in whatever proportion it is mixed with the respirable air; but if it differs only in the first respect, it will hardly cause any sensible error, unless it is more than usually impure, provided care is taken to use such a quantity as is sufficient to produce the full diminution. This has been observed by the Abbé FONTANA, and agrees with my own experiments; for the test of common air tried in my usual method, with some nitrous air which had been debased by the mixture of common air, came out only 18 thousandths less than when tried with air of the best quality, though this air was so much debased that the diminution, on mixing two parts of this with five of common, was one-sixth part less than when good nitrous air

was

was employed; which ſhews, that the error proceeding from the difference of purity of the nitrous air is much leſs when it is uſed in the full quantity than in a ſmaller proportion; and alſo ſhews, that if it is uſed in the full quantity it can hardly cauſe any ſenſible error, unleſs it is more impure than uſual. One does not eaſily ſee, indeed, why it ſhould cauſe any error; for no reaſon appears why the mixture of phlogiſticated or other air, not abſorbable by water, and not affected by reſpirable air, ſhould prevent the nitrous air from diminiſhing and being diminiſhed by the reſpirable air in juſt the ſame manner that it would otherwiſe be. It muſt be obſerved, however, that if the nitrous air is mixed with fixed air, it will cauſe an error, as part of the fixed air will be abſorbed by the water while the teſt is trying; for which reaſon care ſhould be taken that the nitrous air ſhould not be much mixed with this ſubſtance, which it will hardly be, unleſs either the metal it is procured from is covered with ruſt; or unleſs the water in which it is received contains much calcareous earth ſuſpended by fixed air, as in that caſe, if any of the nitrous acid comes over with the air, it will diſſolve the calcareous earth, and ſeparate ſome fixed air.

In order to ſee whether it is poſſible for nitrous air to differ in the ſecond reſpect, I procured ſome from quickſilver, copper, braſs, and iron, and obſerved the teſt of the ſame parcel of common air with them, on the ſame day, making four trials with each, when the difference between the teſts tried with the three firſt kinds of air was not greater than might proceed from the error of the experiment; but thoſe tried with the air from iron were $\frac{1}{100}$ths greater than the reſt. I then took the teſt of ſome more common air with them in the ſame manner, only uſing four parts of common to one of nitrous air,

when

when the teſts tried with the air from iron came out ſmaller than the reſt by not leſs than $\frac{1}{100}$ths. It ſhould ſeem, therefore, from theſe experiments, that the nitrous air procured from iron, beſides being much more impure than the others, differs from them alſo in the ſecond reſpect; that is, that the pure nitrous air in it contains rather leſs phlogiſton than that in the others: whence it happens, that a greater quantity is neceſſary to phlogiſticate a given portion of common air, and conſequently that the diminution is greater when a ſufficient quantity of it is uſed, though with a leſs proportion the diminution is much leſs than with other nitrous air, on account of its greater impurity. As for the air procured from the three other ſubſtances, I cannot be ſure that there is any difference between them. The nitrous air I always uſe is made from copper, as it is procured with leſs trouble than from quickſilver, and I have no reaſon to think it more likely to vary in its quality.

During the laſt half of the year 1781, I tried the air of near 60 different days, in order to find whether it was ſenſibly more phlogiſticated at one time than another; but found no difference that I could be ſure of, though the wind and weather on thoſe days were very various; ſome of them being very fair and clear, others very wet, and others very foggy.

My way was to fill bottles with glaſs ſtoppers every now and then with air from without doors, and preſerve them ſtopped and inverted into water, till I had got ſeven or eight, and then take their teſt; and whenever I obſerved their teſt, I filled two bottles, one of which was tried that day, and the other was kept till the next time of trying, in order to ſee how nearly the teſt of the ſame air, tried on different days, would agree. The experiment was always made with diſtilled water, and care was

always taken to obſerve the diminution which nitrous air ſuffered by being ſhaken in the water, as mentioned in p. 115. The heat of the water in the tub alſo was commonly ſet down. Moſt of the bottles were tried only in the firſt method; but ſome of them were alſo tried by the ſecond, and by the method juſt deſcribed in the manner of FONTANA.

The reſult was, that the teſt of the different bottles tried on the ſame day never differed more than .013, and in general not more than half that quantity. The teſt, indeed, of thoſe tried on different days differed rather more; for taking a mean between the teſts of the bottles tried on the ſame day, there were two of thoſe means which differed .025 from each other; but, except thoſe two, there were none which differed more than .013. Though this difference is but ſmall, yet as each of theſe means is the mean of ſeven or eight trials, it is greater than can be expected to proceed from the uſual errors of the experiment. This difference alſo is not much diminiſhed by correcting the obſervations on account of the heat and abſorbing power of the water, according to the rule in p. 118. This might incline one to think, that the parcels of air examined on ſome of thoſe days of trial were really more dephlogiſticated than the reſt; but yet, I believe, that they were not: for whenever there was any conſiderable difference between the means of two ſucceſſive days of trial, there was nearly the ſame difference between the teſts of the two bottles of the very ſame air tried on thoſe two days. For example, the mean of the trials on July 7. was .016 leſs than that of thoſe on the 15th of the ſame month; but then the teſt of the air caught and tried on the 7th was equally leſs than that of the air of the ſame day tried on the 15th; which ſhews, that this difference between the means of thoſe two days was not owing

to

to the parcels of air tried on the former day being really more dephlogiſticated than thoſe tried on the latter, but only to ſome unperceived difference in the manner of trying the experiment; or elſe to ſome unknown difference in the nature of the water or nitrous air employed. A circumſtance which ſeems to ſhew that it was owing to the firſt of theſe two cauſes is, that it frequently happened, that on thoſe days in which the teſts taken in the firſt method came out greater than uſual, thoſe taken in FONTANA's manner, or in the ſecond method, did not do ſo; the trials, however, made in theſe two methods were too few to determine any thing with certainty. On the whole there is great reaſon to think, that the air was in reality not ſenſibly more dephlogiſticated on any one of the ſixty days on which I tried it than the reſt.

The higheſt teſt I ever obſerved was 1.100, the loweſt 1.068, the mean 1.082.

I would by all means recommend it to thoſe who deſire to compare the air of different places and ſeaſons, to fill bottles with the air of thoſe places, and to try them at the ſame time and place, rather than to try them at the time they were filled, as all the errors to which this experiment is liable, as well thoſe which proceed from ſmall differences in the manner of trying the experiment, as thoſe which proceed from a difference in the nature of the water and nitrous air, will commonly be much leſs when the different parcels of air are tried at the ſame time and place than at different ones; provided only, that air can be kept in this manner a ſufficient time without being injured, which I believe it may, if the bottles are pretty large, and care is taken that they, as well as the water uſed in filling them with air, are perfectly clean. I have tried air kept in the abovementioned manner for upwards of three-quarters of a year

year in bottles holding about a pint, which I have no reaſon to think was at all injured; but then I have tried ſome kept not more than one-third part of that time which ſeemed to have been a little impaired, though I do not know what it could be owing to, unleſs it was that the bottles were ſmaller, namely, holding leſs than one-fourth of a pint, and that in all of them, except two, which were ſmaller than the reſt, the ſtopper which, however, fitted in very tight, was tied down by a piece of bladder.

I made ſome experiments alſo to try whether the air was ſenſibly more dephlogiſticated at one time of the day than another, but could not find any difference. I alſo made ſeveral trials with a view to examine whether there was any difference between the air of London and the country, by filling bottles with air on the ſame day, and nearly at the ſame hour, at Marlborough-ſtreet and at Kenſington. The reſult was, that ſometimes the air of London appeared rather the pureſt, and ſometimes that of Kenſington; but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of days compared was 20, and a great part of them taken in winter, when there are a greater number of fires, and on days when there was very little wind to blow away the ſmoke.

It is very much to be wiſhed, that thoſe gentlemen who make experiments on factitious airs, and have occaſion to aſcertain their purity by the nitrous teſt, would reduce their obſervations to one common ſcale, as the different inſtruments employed for that purpoſe differ ſo much, that at preſent it is almoſt impoſſible to compare the obſervations of one perſon with thoſe of another. This may be done, as there ſeems to be ſo very little difference in the purity of common air at dif-

ferent times and places, by assuming common air and perfectly phlogisticated air as fixed points. Thus, if the test of any air is found to be the same as that of a mixture of equal parts of common and phlogisticated air, I would say, that it was half as good as common air; or, for shortness, I would say, that its standard was $\frac{1}{2}$: and, in general, if its test was the same as that of a mixture of one part of common air and x of phlogisticated air, I would say, that its standard was $\frac{1}{1+x}$. In like manner, if one part of this air would bear being mixed with x of phlogisticated air, in order to make its test the same as that of common air, I would say, that it was $1+x$ times as good as common air, or that its standard was $1+x$; consequently, if common air, as Mr. SCHEELE and LA VOISIER suppose, consists of a mixture of dephlogisticated and phlogisticated air, the standard of any air is in proportion to the quantity of pure dephlogisticated air in it. In order to find what test on the Eudiometer answers to different standards below that of common air, all which is wanted is to mix common and perfectly phlogisticated air in different proportions, and to take the test of those mixtures; but in standards above that of common air, it is necessary to procure some good dephlogisticated air, and to find its standard by trying what proportion of phlogisticated air it must be mixed with, in order to have the same test as common air, and then to mix this dephlogisticated air with different proportions of phlogisticated air, and find the test of those mixtures *.

On

* The rule for computing the standard of any mixture of dephlogisticated and phlogisticated air is as follows. Suppose that the test of a mixture of D parts of dephlogisticated air with P of phlogisticated air is the same as that of common air,

On this principle I found the ſtandard anſwering to different teſts on both my Eudiometers, and alſo on FONTANA's, to be as follows:

Standard.	Teſt by firſt method.	Teſt by ſecond method	Teſt by FONTANA abridged. 1	2	3	4	Total diminu.
4.8	5.02 – –	3.62	.73	.44	.13	1.02	3.98
3.61	3.72 – –	2.70	.75	.49	1.00	– –	3.
2.39	2.55 by large bottle 2.45 by middle bottle	1.87	.76	.96	1.92	– –	2.08
1.00	1.11 by middle bottle 1.08 by leaſt bottle	.89	1.00	– –	– –	– –	1.00
.75	.81 – –	.69	1.23	– –	– –	– –	.77
.5	.57 – –	.51	1.45	– –	– –	– –	.55
.25	.32 – –	.31	1.66	– –	– –	– –	.34
.0	.07 – –	.08	1.94	– –	– –	– –	.06

Standard.	Teſt by FONTANA's method. 1	2	3	4	5	6	7	Total diminu.
4.8	1.75	1.43	1.11	.78	.46	.21	1.18	7.82
3.61	1.75	1.46	1.17	.89	1.16	2.13	– –	5.87
2.39	1.76	1.50	1.25	2.06	– –	– –	– –	3.94
1.	1.81	2.12	3.12	– –	– –	– –	– –	1.88
.75	1.82	2.54	– –	– –	– –	– –	– –	1.46
.5	1.98	2.94	– –	– –	– –	– –	– –	1.06
.25	2.42	3.39	– –	– –	– –	– –	– –	.61
.0	2.91	– –	– –	– –	– –	– –	– –	.09

The phlogiſticated air uſed in theſe experiments was procured by means of liver of ſulphur.

air, then is the ſtandard of the dephlogiſticated air $\frac{D+P}{D}$. Let now ϑ parts of this dephlogiſticated air be mixed with φ parts of phlogiſticated air, the ſtandard of the mixture will be $\frac{D+P}{D} \times \frac{\vartheta}{\vartheta+\varphi}$.

The trials, called FONTANA abridged, were made in the Abbé FONTANA's manner, except that only one meaſure of reſpirable air was uſed, the nitrous air being added by one meaſure at a time as uſual. The column marked 1 at top is the bulk of the mixture after one meaſure of nitrous air was added; that marked 2, its bulk after two meaſures were added, and ſo on.

It muſt be obſerved, that in theſe experiments a conſiderable diminution took place in taking the teſt of the unmixed phlogiſticated air, or that whoſe ſtandard is marked 0 in the table; but, notwithſtanding this, the air, as far as I could perceive, was perfectly phlogiſticated, the diminution being cauſed merely by the abſorbtion of the nitrous air by the water. What ſhews this to be the caſe is, that if common and nitrous air are mixed in ſuch proportions as that the nitrous ſhould be predominant, ſo as to be conſiderably diminiſhed by the mixture of common air, this mixture will produce as great a diminution with nitrous air as the phlogiſticated air uſed in theſe experiments; and if plain nitrous air is added to nitrous air, the diminution is ſtill greater. This ſhews, that a conſiderable diminution is produced by mixing perfectly phlogiſticated air with nitrous air, and alſo that air may be perfectly phlogiſticated by liver of ſulphur.

Theſe experiments alſo ſhew the neceſſity of uſing ſuch a quantity of nitrous air as is ſufficient to produce the full diminution, in order to form a proper eſtimate of the goodneſs of air; for if the quantity of nitrous air is much leſs than that, the air you try will appear very little better than air of a much inferior quality. For example, if in taking the teſt of very good dephlogiſticated air, only an equal bulk of nitrous air is uſed,

uſed, it will appear very little better than a mixture of equal parts of this and phlogiſticated air; and if twice that quantity of nitrous air is uſed, it will appear very little better than a mixture of three parts of this air with one of phlogiſticated. Another great advantage of uſing the full quantity of nitrous air is, that thereby the error ariſing from any difference in its purity is very much diminiſhed.

Perfectly phlogiſticated air may be conveniently procured by putting ſome ſolution of liver of ſulphur into a bottle of air well ſtopped, and ſhaking it frequently till the air is no longer diminiſhed, which, unleſs it is ſhaken very frequently, will take up ſome days. Care muſt be taken, however, to looſen the ſtopper now and then, ſo as to let in air to ſupply the place of the diminiſhed air. In order to know when the air is as much diminiſhed as it can be, the beſt way is, when the air is ſuppoſed to be nearly phlogiſticated, to place the bottle with its mouth under water, ſtill keeping it ſtopped, and to looſen the ſtopper now and then, while under water, ſo as to let in water to ſupply the place of the diminiſhed air, by which means the alteration of weight of the bottle ſhews whether the air is diminiſhed or not. If the ſolution of liver of ſulphur is made by boiling together fixed alkali, lime, and flowers of ſulphur, which is the moſt convenient way of procuring it, the air phlogiſticated by it will be perfectly free from fixed air: whether it will be ſo if the liver of ſulphur is made without lime, I am not ſure.

A ſtill more convenient way, however, of procuring phlogiſticated air is by a mixture of iron filings and ſulphur; and, as far as I can perceive, the air procured this way is as completely phlogiſticated as that prepared by liver of ſulphur.

Where-

Where the impurities mixed with the air have any conſiderable ſmell, our ſenſe of ſmelling may be able to diſcover them, though the quantity is vaſtly too ſmall to phlogiſticate the air in ſuch a degree as to be perceived by the nitrous teſt, even though thoſe impurities impart their phlogiſton to the air very freely. For inſtance, the great and inſtantaneous power of nitrous air in phlogiſticating common air is well known; and yet ten ounce meaſures of nitrous air, mixed with the air of a room upwards of twelve feet each way, is ſufficient to communicate a ſtrong ſmell to it, though its effect in phlogiſticating the air muſt be utterly inſenſible to the niceſt Eudiometer; for that quantity of nitrous air is not more than the 1400000th part of the air of the room, and therefore can hardly alter its teſt by more than $\frac{1}{1400000}$ or $\frac{1}{47000}$th part. Liver of ſulphur alſo phlogiſticates the air very freely, and yet the air of a room will acquire a very ſtrong ſmell from a quantity of it vaſtly too ſmall to phlogiſticate it in any ſenſible degree. In like manner it is certain, that putrifying animal and vegetable ſubſtances, paint mixed with oil, and flowers, have a great tendency to phlogiſticate the air; and yet it has been found, that the air of an houſe of office, of a freſh painted room, and of a room in which ſuch a number of flowers were kept as to be very diſagreeable to many perſons, was not ſenſibly more phlogiſticated than common air. There is no reaſon to ſuppoſe from theſe inſtances, either that theſe ſubſtances have not much tendency to phlogiſticate the air, or that nitrous air is not a true teſt of its phlogiſtication, as both theſe points have been ſufficiently proved by experiment; it only ſhews, that our ſenſe of ſmelling can, in many caſes, perceive infinitely ſmaller alterations in the purity of the air than can be perceived

Fig. 4.

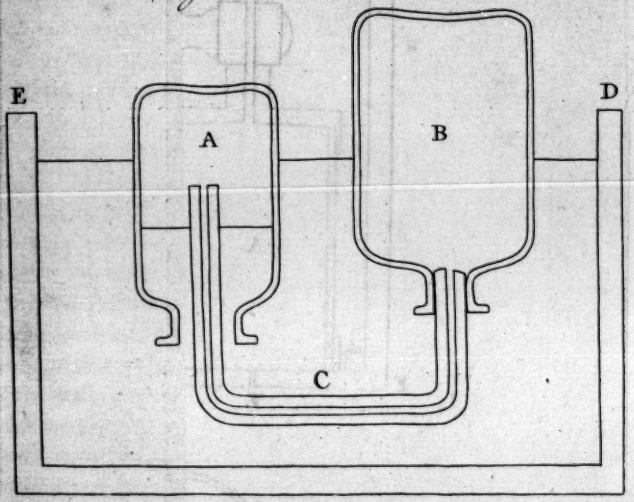


Fig. 1.

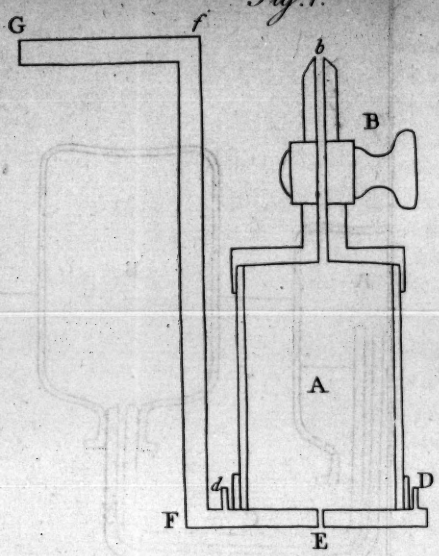


Fig. 2.

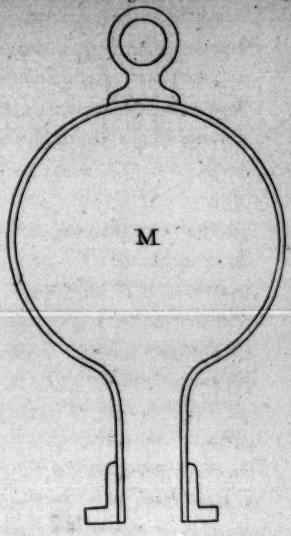
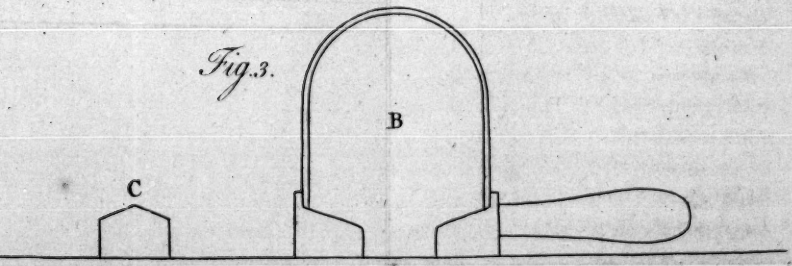


Fig. 3.



ceived by the nitrous teſt, and that in moſt rooms the air is ſo frequently changed, that a conſiderable quantity of phlogiſticating materials may be kept in them without ſenſibly impairing the air. But it muſt be obſerved, that the nitrous teſt ſhews the degree of phlogiſtication of air, and that only; whereas our ſenſe of ſmelling cannot be conſidered as any teſt of its phlogiſtication, as there are many ways of phlogiſticating air without imparting much ſmell to it; and, I believe, there are many ſtrong ſmelling ſubſtances which do not ſenſibly phlogiſticate it.

IX. *Experiments upon the Reſiſtance of the Air.* By Richard Lovell Edgworth, *Eſq. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 16, 1782.

DEAR SIR,

Edgeworth's Town, Ireland, May 1, 1782.

THE laſt time I had the pleaſure of ſeeing you, the converſation turned upon the reſiſtance of the air, and upon a ſingular experiment related by Mr. ROBINS in his Treatiſe upon Gunnery. I have ſince repeated his experiment, and tried ſome others, which I beg you to lay before the Royal Society, if you think them worthy their attention.

I have the honour to be, &c.

MANY experiments have been tried to aſcertain the force and velocity of the wind, with a view to the conſtruction and management of different engines, and more particularly for the purpoſes of navigation. Several machines, which have been employed in theſe enquiries, are deſcribed in the Tranſactions of the Royal Society, and in the Memoirs of Foreign Academies; but the moſt accurate which I have ſeen was invented by the late Sir CHARLES KNOWLES; and from a number of experiments made with it, he had conſtructed tables, ſhewing at one view the force of the wind upon each ſail of a ſhip at every degree of velocity from one to ninety miles an hour.

But

But these calculations, and many more of a similar nature, that are to be met with in BELIDOR's Architecture Hydraulique, and other books, are founded upon a supposition that the effect of the wind is directly as the surface upon which it acts. If, for instance, its force be estimated as one upon one square yard, its force upon two square yards should be estimated as two, upon three square yards as three, &c.; but in fact this proportion is not to be depended upon, nor must the resistance of surfaces be estimated merely by their extent; but several other circumstances must be taken into consideration.

No figures can resemble each other more than a parallelogram and a square having the same superficial contents, as they are both bounded by four straight lines meeting at right angles, yet they oppose different degrees of resistance to the air.

If two similar cards, for instance, are placed opposite the wind, one upon its end, and the other on its side, and both inclined to the same angle, the wind will have the greater effect upon the card that is placed end-ways.

To determine the difference of resistance between these two surfaces, and to ascertain the effect of other figures moving through the air, I tried the following experiments. The two first are to be found in Mr. ROBINS's Treatise upon Gunnery; but I thought it proper to repeat them, that they might be more readily compared with others made with the same apparatus, especially as Mr. ROBINS made use of a machine constructed upon a smaller scale than mine, and turning upon friction wheels, which are not proper for machines of this nature, nor indeed for any purpose, where an uniform motion is required.

Having faſtened a ſtrong joiſt of wood from one ſide of a large room to the other, ſo as to form a kind of bridge at ſome diſtance from the floor, I erected a perpendicular ſhaft or roller, which turned freely in braſs ſockets fixed into the floor and bridge upon pivots of hardened ſteel one-ſixteenth of an inch in diameter. On each ſide of this roller was extended an arm of deal, feather-edged, and ſupported by ſtays of the ſame material, feathered in the ſame manner, to oppoſe as little ſurface as poſſible to the air when in motion.

Round the upper part of this roller was wound a ſtring of cat-gut, which, paſſing over pullies properly diſpoſed, was faſtened to a ſcale that deſcended into the well of an adjoining ſtair-caſe.

The extremity of theſe arms deſcribed a ſpace of more than forty feet in every revolution, the weight deſcending in the ſame time only ſix inches. The time in all the following experiments was the ſame; and, as each revolution was performed in four ſeconds, the velocity of the end of the arm on which the ſurface was fixed, was at the rate of about ſeven miles an hour.

The firſt figure that I tried was a parallelogram of tin, nine inches long, and four inches wide. Its longeſt ſide was placed parallel to the floor, at the extremity of one of the arms. Its ſhorteſt ſides were inclined to an angle of forty-five degrees from the perpendicular, and in this ſituation it was carried round with its ſurface againſt the air.

After ſuffering it to revolve until I was ſatisfied that its motion was become uniform, I put as much weight into the ſcale as moved it with a velocity of five turns in twenty ſeconds. I then changed the ſituation of the parallelogram, placing its ſhorteſt ſides parallel to the floor, and inclined to the ſame

angle as before. I now found, that more weight was required to produce the ſame velocity, though the quantity of ſurface was the ſame as in the preceding experiment. The weight neceſſary to put the machine alone in motion, with the velocity above mentioned, was two pounds and a half. When it carried the parallelogram with one of its ſhorteſt ſides downwards, it required four pounds and a half additional weight; and when the parallelogram was reverſed, another half pound was barely ſufficient to give it the ſame velocity.

The difference, therefore, occaſioned by placing the ſame parallelogram with its longer or ſhorter ſides inclined from the direction of its motion was equal to one-tenth of the greateſt reſiſtance.

It has been obſerved, that in theſe two experiments the mean velocity of the plane was not the ſame, as its extremity extended farther from the center of the machine in one than in the other. This is ſtrictly true; but the ſize of the parallelogram bore ſo ſmall a proportion to the length of the radius to which it was faſtened, that the error ariſing from this circumſtance is ſcarcely perceptible, and the advantage being in favour of that which required the leaſt weight, I did not think it neceſſary to bring it into account.

Having formed a general idea of the reaſon of the difference in theſe experiments, it occurred to me, that there would be a greater diſproportion between the reſiſtance of ſome other figures, which Mr. ROBINS had not tried; and having put a rhomboid, in the form of a lozenge, nine inches long, and four broad, in the place of the parallelogram, the difference was increaſed from one-tenth to one-ſeventh of the weight employed to give them the required velocity.

Purſuing the ſame reaſoning that led me to the laſt experiment, it occurred to me, that even againſt figures of exactly the ſame ſhape, the reſiſtance of the air, when the dimenſions of the figures were enlarged, would not be increaſed in the ſame proportion as the ſize of the planes, but in a much higher ratio; and that, by bending the planes as a ſail, the reſiſtance would be ſtill farther increaſed, though the ſection of air, that would be intercepted by the planes, muſt by theſe means be conſiderably leſſened.

The reſult far ſurpaſſed my expectations. A ſquare of tin, containing ſixteen ſquare inches, placed perpendicularly, was reſiſted as two and a half. A ſquare, containing ſixty-four inches, or four times the former quantity, inſtead of meeting with a reſiſtance as ten or four times the former reſiſtance, required no leſs than fourteen pounds to give it the ſame velocity.

Four-tenths (or nearly half as much again) was an increaſe of reſiſtance that made me ſuſpect ſome error in the experiment; but having repeated it ſeveral times with great care, and having examined all the parts of the machine, I was ſatisfied that I had made no miſtake.

I now placed the parallelogram of nine inches long upon the arms of the machine, with its ſhorteſt ſides parallel to the horizon, bending it to ſuch an arch that its chord meaſured eight inches, and inclining it to an angle of forty-five degrees. And though the ſection of air that it intercepted was by theſe means diminiſhed one-ninth, yet the reſiſtance was increaſed from five to five and a half. And when the parallelogram was bent yet farther, and its chord contracted almoſt to ſeven inches, the reſiſtance was increaſed to five and three-quarters.

I mention theſe numbers in groſs to avoid confuſion; but in the

the table at the end of this paper, the meaſures and weights are ſet down exactly.

Dr. HOOK, whoſe name muſt be reſpected by every experimental philoſopher, was aware, that although he thought he could demonſtrate that flat ſails were preferable to ſuch as were curved and hollowed by the wind, yet until proper experiments had been tried, nothing could be poſitively determined.

He ſays ſomewhere in his poſthumous works, "That he "was ſurpriſed at the obſtinacy of ſeamen, in continuing, "after what appeared the cleareſt demonſtration to the con- "trary, to prefer bellying or bunting ſails to ſuch as were "hauled taught; but that he would, at ſome future time, "add the teſt of experiment to mathematical inveſtigation." He reaſoned upon a ſuppoſition, that the air in motion followed the ſame laws as light; and that it was reflected from ſurfaces with the angle of reflection equal to the angle of incidence, which is not the caſe, as it never makes an angle with the plane, but is always reflected in curves. Monſ. PARENT, and other mathematicians, have fallen into the ſame miſtake. No demonſtration of this ſort was more commonly known or received among practical mechanics, than that the beſt angle for the ſails of a wind-mill, at the beginning of their motion, was an angle of forty-five degrees; and that the maximum of an under-ſhot water-wheel was when it moved with one-third of the velocity of the water: but Mr. SMEATON, in an excellent paper in the Philoſophical Tranſactions, has refuted this opinion by the cleareſt experiments.

I had intended to diverſify theſe experiments, and to extend them to a more intereſting ſubject of enquiry, to determine the beſt ſhape of ſails, and the angle to which they ſhould be ſet, to obtain the greateſt progreſſive effect with the leaſt lee-

way; but, as a more complicated apparatus than I could at preſent procure is neceſſary for this purpoſe, I determined to offer you the ſlight progreſs I have made, in hopes that ſome gentleman, more converſant and more intereſted than myſelf in theſe inquiries, may purſue them with ſucceſs and advantage to the public. I ſhall only remark, that the general cauſe of the different reſiſtance of the air upon ſurfaces of different ſhapes, is the ſtagnation of that fluid near the middle of the plane upon which it ſtrikes. The ſhape and ſize of the portion thus ſtagnated, differs from the ſhape and angle of the plane. The elaſticity of the air permits the parts in motion to compreſs thoſe which are firſt ſtopped or retarded by the plane, and forms, as it were, a new ſurface of a different ſhape, for the reception of thoſe particles which ſucceed. With the aſſiſtance of a good ſolar microſcope the curves of the air ſtriking againſt different ſurfaces may be delineated, and when the general facts are once clearly aſcertained, mathematicians will have an ample field for curious and uſeful ſpeculation.

TABLE.

TABLE.

	Turns.	Time.	Weight.
Machine alone - -	5	4	2 8
With a parallelogram of nine inches long and four broad, one of its longeſt ſides, parallel to the horizon and the parallelogram, inclined to an angle of 45°, -	5	4	7 0
Ditto, with one of its ſhorteſt ſides downwards, - - -	5	4	7 9
With a lozenge nine inches long and four broad, with its longeſt ſide parallel to the horizon, - - -	5	4	5 8
Ditto reverſed, - - -	5	4	6 0
With a ſquare piece of tin, four inches by four inches, - - -	5	4	5 0
Ditto, eight inches by eight inches,	5	4	16 6
With the former parallelogram, placed with one of its ſhorteſt ſides downwards, inclined to an angle of 45°, and bent into an arch whoſe chord was eight inches long, - - -	5	4	8 0
Ditto bent to an arch, the chord of which was ſeven inches and a quarter, -	5	4	8 5

X. *An Anſwer to the Objections ſtated by M.* De la Lande, *in the Memoirs of the* French *Academy for the Year* 1776, *againſt the Solar Spots being Excavations in the luminous Matter of the Sun, together with a ſhort Examination of the Views entertained by him upon that Subject. By* Alexander Wilſon, *M. D. Profeſſor of Practical Aſtronomy in the Univerſity of* Glaſgow; *communicated by* Nevil Maſkelyne, *D.D. F. R. S. and Aſtronomer Royal.*

Read January 16, 1783.

TO THE REV. DR. MASKELYNE.

DEAR SIR, Glaſgow College, April 20, 1782.

HAVING lately ſeen a paper on the Solar Spots by M. DE LA LANDE, in the French Memoires for 1776, publiſhed in 1779, I found ſuch mention made of mine upon that ſubject, as appears to me to require an anſwer; not only in juſtice to myſelf, but to that honourable Society who admitted an account of my diſcovery into their Tranſactions, and to philoſophy in general, whoſe cauſe it becomes a duty upon her followers to vindicate from ſuch errors as ſeem likely to prevail.

The univerſal circulation of the volumes of the French Academy makes me the more ſolicitous to be heard in anſwer; and to have that anſwer conveyed to the public through the channel

of

of the Philosophical Transactions, as being the only one where an appeal can be made equally general and extensive.

It has been my aim to draw up the paper, which at this time I take the liberty of transmitting, with that freedom which the important interest of truth demands, and to observe at the same time those rules of decorum, of which too it becomes philosophers, of all other men, to set the example.

Give me leave, therefore, to request, as a very particular favour, that you will be so good as to communicate the inclosed to your colleagues in the Council of the Royal Society; as also this letter, in which I wish to express the most unreserved deference to your and their opinions and determination.

I am, &c.

MAC FARLANE Observatory, April 18, 1782.

IN the first part of my paper, published in the Philosophical Transactions for 1774, I have explained how, from the lucky accident of seeing the great solar spot of November 1769, in a certain critical situation upon the disk, its real nature was obtruded on my thoughts by a train of appearances the most obvious and unequivocal. The reader may there also see how, from phenomena perfectly similar in spots of the usual size, I was led to a general conclusion, and to believe that all spots, small as well as great, which consist of a dark nucleus, and surrounding umbra, are excavations in the luminous matter of the sun.

Before this time no opinions had been entertained of any third dimension belonging to the spots, but what agreed to the conception of their being something material extending more

or less above the common level of the sun's spherical surface. This idea, though so very prevalent, seems however to have originated rather from pre-conceived theoretical notions of the nature and constitution of that vast body, than from any phenomena of a constant and marked kind observable in the spots themselves.

The views which I have offered upon this subject disclaim, I believe, for the first time all alliance with hypothesis, and seek after a permanence in the strong holds of no imaginary system. They make a direct appeal to our senses, upon the testimony of which alone their merits must finally rest.

As for myself, situations will sometimes occur where it would be as inconsistent with the spirit of the most guarded philosophy to harbour doubts, as on other occasions to be weakly credulous. When in the year 1769 I first mentioned the discovery in the London Chronicle, and even at the time of sending a full account of it to the Royal Society, I considered it as too early to offer my sentiments in a manner intirely decided. But in the lapse of eight more years the subsequent observations have all conspired to strengthen, and even to perfect, my convictions. Now, therefore, I have no hesitation in pledging any credit which may belong to me, as an astronomical observer, to the present and to future times, for the reality of that discovery which is laid down in the first part of the paper above mentioned.

But though I maintain that any person, with a good telescope, and with good eyes practised in observing, cannot fail of beholding frequently in the sun the phenomena I have pointed out, it yet may require the more palpable dimensions of some future great spots, in order to satisfy people less accustomed to examine and to judge of objects by means of glasses. This is

the more to be expected, as I find, that even from amongſt thoſe the moſt profoundly ſkilled, a demand has lately been made upon me to uphold what I have advanced, and to remove what ſeem to be ſtated as unſurmountable objections.

Having ſome little time ago ſeen the Memoirs of the French Academy for the year 1776, publiſhed in 1779, I there find my paper on the Solar Spots has come under the notice of a member of that illuſtrious body, whoſe name is juſtly held in great eſteem by all aſtronomers, and to whom aſtronomy itſelf has many obligations. The author alluded to is M. DE LA LANDE. Though I ſhould have been much flattered to have found my views ſupported by an authority ſo truly reſpectable, yet, even in his endeavours to oppoſe me, I honour him as a philoſopher who has taken ſo much pains to vindicate what he doubtleſs believes to be juſter opinions. In the moſt perfect confidence of a generous indulgence on his part, and with equal attachments to philoſophy and to truth, my preſent intention is very freely to offer what arguments occur to me in favour of the ſolar ſpots being ſuch as I have deſcribed.

Firſt of all, it has been urged, as an objection of great weight, that the abſence of the umbra on one ſide, when ſpots are near the limb, as ſo fully explained in my paper, is not conſtant. As to the fact, the reader may there ſee, that I was myſelf ſufficiently aware of it, having ſtated three caſes from my own obſervations, when I did not perceive this change to take place. The rev. FRANCIS WOLLASTON, LL.B. F. R. S. is the only perſon who (in the Philoſophical Tranſactions) has beſtowed any remarks on my publication; and though he with great candour acknowledges that, generally, the umbra changes in the manner I have determined, yet he expreſſes a difficulty as

to my concluſions, on account of this circumſtance not obtaining univerſally.

Under ſimilar expreſſions M. DE LA LANDE, in the Memoire before me, produces from his own obſervations, which appear to have been long continued, only three caſes of the ſame kind, and from the ancient obſervations of Meſſ. CASSINI and DE LA HIRE, four more. In regard to theſe laſt, I am not ſure if ſuch obſolete ones ought to be referred to in a queſtion of the preſent kind. Thoſe excellent obſervers, entertaining no thoughts that any thing of moment depended upon a nice attention to the form of the ſpots, might eaſily overlook leſs obvious circumſtances, eſpecially when they were found near the limb. We may add further, that, even when ſo ſituated, they retain the umbra at both ends, and that whole ſide of it which lies fartheſt from the center of the diſk and theſe parts in the aggregate, they might ſometimes miſtake for the umbra as not deficient in any particular place. That they did not affect what muſt have appeared to them a needleſs refinement in accuracy may be collected from the following expreſſion of M. DE LA HIRE, found in the Mem. Acad. 1704, p. 10. As to this ſpot, ſays he, "Je ne donne point ici, les figures differentes ſous "leſquelles cette tache a paru;" and, among other reaſons for this, he gives the following: "Car il me ſemble qu'on ne "peut pas tirer d'utilité de ces ſortes de figures qui changent "continuellement."

But even admitting the anomaly we at preſent conſider to be much more frequent than can be contended for, ſtill ſuch caſes can only be brought as ſo many exceptions from a certain *general law*, or uniformity of appearance, from which the condition of by far the greater number of ſpots is moſt undeniably deduced. The utmoſt, therefore, that can hence be alledged

is, that ſome few ſpots differ from all the reſt, or from the multitude, and are not like theſe excavations in the ſun. Such caſes or exceptions will not ſurely warrant the concluſion, that no ſpot can be an excavation. This would be to reverſe all the rules of a juſt induction, by oppoſing to an irrefragable general argument, the force of one extremely limited and feeble.

But notwithſtanding theſe few inſtances where the umbra is not found to change, when we conſider how perfectly all ſpots reſemble one another in their moſt ſtriking features, there naturally ariſes ſome preſumption for all under that deſcription we have given partaking of one common nature; and for this only difference in the phenomena depending upon ſomething which does not neceſſarily imply a complete generical diſtinction.

It comes therefore to be inquired, how far ſpots, which when near the middle of the diſk appear equal and ſimilar in all things, may yet differ from one another conſidered as excavations, or as poſſeſſing the third dimenſion of depth, and how far the peculiar circumſtances by which they may diſagree can contribute to make ſome reſiſt this change of the umbra, when near the limb, much more than others.

In order to this, ſuppoſe two ſpots which occupy a ſpace upon the ſun correſponding to the equal arches GD (fig. 1.); and let GM, DM, be drawn ſo as to coincide with the plane of the excavation in ſuch caſe. The breadth of the nucleus being commonly equal to that of the ſurrounding umbra, if the baſe MD of the triangle GDM conceived rectilineal be divided in L, ſo as ML : LD :: MD : DG; and if through L be drawn LS parallel to DG, then will DGSL be the ſection of two ſpots having this condition, and which as to ſenſe would, when far away from the limb, be equal in all apparent meaſures;

meaſures; though very unequal in the third dimenſion HE or depth of the nucleus SL, and alſo in the inclination DGM of their ſides to the ſpherical ſurface of the ſun. Now it is manifeſt, from the conſtruction of the figure, that the diſtances AB, AK, from the limb A, when the ſides GS of the umbra diſappear, muſt depend very much upon the laſt of theſe two circumſtances; and that, according as the angle of inclination DGM is leſs, the reſpective ſpot will go nearer to the limb than the other before the ſide of the umbra GS vaniſhes. But thoſe very exceptions to the general phenomena which we are at preſent examining are of this kind, and may, perhaps, from what has been now ſhewn, proceed wholly from the ſhallowneſs and the very gradual ſhelving of ſome few ſpots which break out in certain tracts of the ſun's body over which the luminous matter lies very thinly mantled.

If, therefore, upon ſuch principles it can be ſhewn, that ſpots, ſimilar to the reſt, may ſometimes go to the limb without the one umbra contracting ſenſibly more than the other, the objection we are at preſent conſidering will be entirely removed, and it will be allowable to conclude, that even theſe few ſpots are excavations like all the reſt, though ſhallower, as it would be quite unphiloſophical to multiply diſtinctions concerning their nature, where there is found no neceſſity for ſo doing.

In order to avoid circumlocution, we may call that ſide of the umbra which lies neareſt the limb the *neareſt umbra*, and the ſide oppoſite the *fartheſt umbra*; and to enter more particularly into the conſideration now before us, let us ſuppoſe a ſpot of 40″ over all, with its nucleus and umbra equally broad; then will the depth of the nucleus and the apparent breadth of the neareſt umbra, when the plane of the fartheſt comes to coincide with the viſual ray, be as expreſſed in the following examples,

examples, in which the apparent ſemi-diameter of the ſun is ſuppoſed 16′, and his parallax 8″.5.

	Fartheſt umbra ſuppoſed to vaniſh when diſtant from the limb.	Depth of nucleus in Engliſh miles and in ſeconds.	Apparent breadth of neareſt umbra.
	′ ″	″	
I.	1 0	4.54 - 2118	8.58
II.	0 30	3.09 - 1442	6.02
III.	0 15	2.09 - 975	4.13
IV.	0 8	1.44 - 672	2.87

Now, becauſe in every aſpect of a ſpot, the real breadth of either the fartheſt or neareſt umbra muſt be to the projected or apparent breadth, as radius to the ſine of the angle which this reſpective plane makes with the viſual ray, it follows, that at any time before the ſpot comes ſo near the limb as is expreſſed in the above examples, the apparent breadth of the neareſt and fartheſt umbra cannot differ ſo much as by the quantity there ſet down for the apparent breadth of the neareſt, when the other is ſuppoſed to vaniſh. Regarding, therefore, the fartheſt and neareſt umbra of the ſpot in caſe IV. as two neighbouring viſible objects which turn narrower by degrees as the ſpot goes toward the limb, we ſhould undoubtedly judge that they contract as to ſenſe alike, ſince ſo long as the fartheſt could be perceived, the other cannot appear to exceed it by a quantity that we could diſtinguiſh; and by the time the plane of the former coincides with the viſual ray, the extreme nearneſs to the limb would prevent our forming any certain judgement of either.

From this laſt example, therefore, it appears manifeſt that a ſpot, anſwering to the deſcription and conditions there mentioned, or one a little more ſhallow, would approach the limb, and

and finally go off the diſk, without that peculiar change of the umbra on one ſide, which is ſo obvious on common occaſions, notwithſtanding it were an excavation, whoſe nucleus or bottom is ſo many miles below the level of the ſurface.

In the four caſes above ſtated, the diſtance of the remoteſt part of the nucleus from the ſun's limb when the viſual ray coming from it is juſt interrupted by the lip of the excavation, or, in other words, the diſtance of the nucleus from the limb when it is totally hid was alſo computed. Theſe diſtances are as follow:

Caſe 1.	16.93	Caſe 3.	4.70
2.	8.90	4.	2.70

and it is remarkable from the two laſt, how very near the limb a ſhallow ſpot of not more than 40″ in diameter may come before the nucleus wholly diſappears.

Computations of this kind are very eaſily made, by ſuppoſing on (fig. 2.) GDLS a ſection of a ſpot ſo near to the limb A, that the viſual ray VB coincides with GS the plane of the excavation. Let the ſtraight line DF coincide with the other ſide, and draw the radii CA, CQ, ſo as to be at right angles to GB, DF, and draw the radius CH through the point M.

Now, becauſe the verſed ſine AB, the apparent diſtance from the limb, when the ſide of the umbra GS vaniſhes is given, the arch GA of the ſun's circumference is given, and from the known breadth of the ſpot, the arch GD and its half GH are each of them given, and conſequently the arches HA, DA, and QA, are all given. From theſe data the angles and ſides of the triangle GMD, ſuppoſed rectilineal, may be deduced, alſo HM the diſtance of the point of interſection M from the ſurface. When theſe particulars have been determined for any aſſumed diſtance BA, and aſſumed extent of the

ſpot

ſpot GD, the depth of the nucleus HE, may be found by the following analogies.

By ſimilar triangles MD : DG :: ML : LS. But LS being the breadth of the nucleus is, by hypotheſis, equal to LD, the breadth of the umbra; therefore MD : DG :: ML : LD, or as ME : EH on account of the parallels SL and GD. By compoſition, MD + DG : DG :: MH : HE. Hence HE, the only unknown quantity of theſe proportionals, is found, and is the depth of the nucleus ſought. If DP, the apparent breadth of DL, the ſide of the umbra next the limb be required, correſponding to the preſent aſpect of the ſpot, this may be derived from the data by the reſolution of the triangle DLP, whoſe hypothenuſe DL and angle DLP, are known. Again, if the neareſt apparent diſtance of the nucleus from the limb, when it wholly diſappears, be ſought, it will come out equal to the verſed ſine of the arch GA, when diminiſhed by half the arch HA, which laſt is by conſtruction equal to half of either of the acute angles of the triangle DGM. In order to ſee the reaſon of this, it muſt conſidered, that the ſegments ML, LD, of the baſe MD, are in the ſame ratio with the ſides DG, GM; on which account a ſtraight line drawn from G to L, the laſt part of the nucleus which can be ſeen, muſt biſect the angle DGM. Therefore, before the viſual ray or line ZGL can fall perpendicularly upon the radius CA, this muſt be drawn diſtant from the point A towards D, by an arch correſponding to half the angle DGM, that is, by an arch equal to half of HA.

Perhaps it may be urged, that very ſhallow ſpots ought always to be known from the reſt, and to diſcover themſelves, by a ſurrounding umbra very narrow compared to the extent of the nucleus; but we know far too little of the qualities of the luminous matter, and of the proximate cauſes of the ſpots,

to ſay any thing at all upon a point of this kind. The breadth of the umbra is, as aſſumed in the computations, commonly about equal to that of the nucleus, though ſometimes it varies more or leſs; but how far theſe relative dimenſions indicate depth or ſhallowneſs muſt be expounded only by obſervation, and not by any vague or imperfect notions of the nature and conſtitution of the ſun.

The mention of a pit or hollow or excavation ſeveral thouſands of miles deep, reaching to that extent down through a luminous matter to darker regions, is ready to ſtrike the imagination in a manner unfavourable to a juſt conception of the nature of the ſolar ſpots as now deſcribed. Upon firſt thoughts it may look ſtrange, how the ſides and bottom of ſuch vaſt abyſſes can remain ſo very long in ſight, whilſt by the ſun's rotation they are made to preſent themſelves more and more obliquely to our view. But when it is conſidered, how extremely inconſiderable their greateſt depth is, compared to the diameter of the ſun, and how very wide and ſhelving they are, all difficulties of this ſort will be entirely removed.

Unleſs, however, we duly attend to theſe proportions, our notions upon the ſubject muſt be very erroneous; and it ſeems the more neceſſary to offer this caution, as this very thing is inaccurately repreſented in fig. 9. belonging to the Memoire under review, and in a way that may lead to miſtakes. Inſtead of exhibiting a ſpot as depreſſed below the ſurface of the ſun one hundredth part of his ſemi-diameter, the ſection of it is there determined by two lines drawn from the circumference, and meeting in a point at the prodigious diſtance of one-fifth of the ſemidiameter below. This author's known clear and comprehenſive ideas of every thing relating to the ſphere have doubtleſs led him to think, that any particular attention to exactneſs was unneceſſary in this repreſentation;

but

but as my deſign, on the preſent occaſion, is to write and to explain matters in a popular way, rather than to aſtronomers, it will be proper to aſſiſt the conceptions of thoſe who are but little verſed in mathematical principles by ſuch diagrams as will ſhew things in their juſt proportions. Any reader, therefore, who pleaſes, by turning to fig. 3. may ſee how very ſmall a portion of the ſun's body is made up of the luminous matter when ſuppoſed every where 3967 Engliſh miles deep. Fig. 3. A is a ſection of a ſpot of 50″ diameter ſituated in the deepeſt part of this reſplendent ſubſtance.

For my own amuſement I have purſued this ſubject further in the way of ocular proof, by a model of the ſun and of the ſpots upon his body according to their proper dimenſions. This I put into a convenient wooden frame, and viewed it afar off when ſet upon a ſtand, whilſt the globe was turned ſlowly round, and ſubtended an angle at the teleſcope equal to the apparent diameter of the ſun. By an object-glaſs micrometer I then took the diſtances from the limb when the fartheſt umbræ of different ſpots vaniſhed, as alſo the diſtances of the nuclei juſt when diſappearing. The apparent ſubtenſe of the umbra next the limb was alſo meaſured in this way, together with the viſible extenſion of ſome great ſpots within the diſk, when the extreme limits of the neareſt umbra coincided with the limb. In all theſe experiments the effect was very ſtriking, and the phenomena remarkably conſonant to calculation, and to what I have often ſeen upon the real ſun in the heavens.

The globe I got made conſiſts within of two ſtrong hollow hemiſpheres, formed by paſting ſlips of paper upon a well-turned ball of wood, and afterwards faſtened together upon an iron axis in the way commonly practiſed. Over this were repeatedly laid coats of Spaniſh white and glue, applied when in a thick paſte, till at length this outward ſhell

became of a conſiderable thickneſs. To give the whole a true form, the two projecting poles were locked up in two grooves when coinciding with the diameter of an iron ſemi-circle, whoſe inner edge was ſo faſtened as to cut away the redundant parts of the laſt coat of the chalk nearly dry, whilſt the globe was continuedly and ſlowly forced round. By thus repeatedly paring off the protuberances, and ſupplying new paſte when deficient, and forcing the globe round againſt the cutting edge as before, it at length became quite ſmooth and ſpherical. After this, when ſlowly dried, it turned very white, and then the ſpots or excavations were made in its ſurface by boring inſtruments of ſteel as in fig. 4. conſtructed in all their cutting edges from a ſcale of parts of the globe's diameter. This done, I penciled the bottom of the hollows all over black with China ink, and diſtinguiſhed the ſhelving ſides from the full whiteneſs of the outward ſurface by a ſhade of the pencil which was darkeſt towards the external border. I hope the indulgent reader will excuſe me for having been ſo particular in regard to this artificial ſun, as poſſibly what I have mentioned may facilitate a like conſtruction, ſhould any perſon deem it worth while to entertain himſelf with ſuch experiments.

But to proceed; what has now been inſiſted on at ſo much length concerning the ſhallowneſs and the more gradual ſhelving of ſome few ſpots, will alſo apply to another objection, which M. DE LA LANDE views in a ſtrong light.

Here we find quoted the great ſpot in 1719, ſeen by M. CASSINI; and, for the ſecond time, that of June 3, 1703, ſeen by M. DE LA HIRE; both which, on their arrival at the limb, are ſaid to have made an indentation or dark notch in the diſk; and this phenomenon is mentioned as abſolutely incompatible with ſpots being below the ſurface.

It

It is moſt true, that if we look for any thing like this, when the plane which coincides with the external boundary of the ſpot paſſes through the eye (the way that M. DE LA LANDE conſiders the matter, vide his fig. 9th) it muſt be very large indeed before the diſk could be perceived deficient by any dark ſegment. But may not a ſpot, even no larger than M. CASSINI's, conſidered as an excavation, make, in a manner very different from this, ſomething like a notch; for, by the way, this phenomenon is not in the Mem. Acad. nor any where elſe, that I know of, deſcribed with any ſort of preciſion.

M. CASSINI's great ſpot, by which we underſtood the nucleus, was one of 30″; and ſuppoſing the umbra equally broad, its diameter over all muſt have been 1′ 30″. It would, therefore, occupy an extent upon the ſun's ſurface of 5° 22′ fully. Now, ſuppoſe a circular ſpace of that ſize upon the ſun, diſtinguiſhed from the ſurrounding luſtre by ſuch a failure of light as is peculiar to ſome ſpots, and ſuppoſe that it juſt touches the limb, it would ſtill ſubtend an angle of more than 4″. This being the caſe, might not a duſky ſhade, more or leſs remarkable, according to the darkneſs of the umbra, commencing at the limb, and reaching inwards upon the diſk, or, in other words, a notch be perceived? Had M. CASSINI's ſpot been a very ſhallow excavation, it appears by caſe fourth, formerly ſtated, that when viewed in this aſpect, ſome ſmall part of the nucleus might have been yet viſible, and might have contributed along with the ſhade of the fartheſt umbra, and the ſtill deeper and broader ſhade of the two ends of the umbra, to mark out the indentation.

Should it be ſaid, that theſe notches are always diſtinct jet black impreſſions on the diſk, of an obvious breadth, and originating entirely from the opaque nucleus conceived as ſomething

thing prominent above the general ſurface, this can be ſhewn inconſiſtent with ſome circumſtances we find accidentally mentioned in the caſe of M. DE LA HIRE's ſpot: for of this great one, it is ſaid, that when only 8″ diſtant from the limb, the nucleus was ſeen as a very narrow line. This was on June 3, 1703, at ſix o'clock in the morning. Now, foraſmuch as, at that time, its alledged elevation muſt have been to its apparent ſubtenſe, very nearly, as radius to coſine of that arch of the ſun's circumference whoſe verſed ſine was the 8″ of diſtance from limb, it is impoſſible that its breadth could have increaſed ſenſibly in its further progreſs towards the limb; and how any obvious black notch could be produced by the elevation contended for in this caſe, is not conceivable.

I do not imagine, therefore, that the phenomena of notches in the diſk, ſo inconſiderable and dubious as theſe ſeem to be, are by any means a proof of projecting nuclei, or that they are not reconcileable to ſpots being depreſſions in the ſun. A large ſhallow excavation, with the ſloping ſides or umbra darker than common, may, as has been ſhewn, be more or leſs perceptible at the limb: and what, perhaps, is a further confirmation of this, and ſeems to evince that a concurrence of ſuch circumſtances is neceſſary, is, that ſometimes even large ſpots make no indentation. M. CASSINI, in Mem. Acad. tom. X. p. 581. deſcribes the great ſpot of 1676, which he ſaw at its entrance with a teleſcope of 35 feet, as an obſcure line parallel to the limb, but no where mentions that it made a notch in it.

Though we now and then ſee the ſurrounding umbra darker than at other times, yet when ſpots are deep, and the umbræ but little duſky, it is indeed impoſſible that we ſhould ſee any thing of them, even though large, very near the limb: for here even the nucleus which lies buried cannot in the leaſt

contribute to the effect, as it may do a little before its ſtate of evaneſcence when ſpots are very ſhallow: accordingly caſes of this kind are perfectly agreeable to experience. This fact ſeems to have met SCHEINER as a ſtumbling-block, when he was intent upon bringing forward every poſſible argument for the ſpots being ſomething which project beyond the ſurface. "Non "raro contingit," ſays he, Roſa Urſina, p. 511. "ut magnæ "ſolis maculæ antequam ad horizontem accedant, repente ita "luce obruantur, ut omnino videri nequeant."

In reaſoning concerning the nature of the ſpots, and particularly about their third dimenſion, the only arguments which are admiſſible, and which carry with them a perfect conviction, are thoſe grounded upon the principles of optical projection. If, for example, by far the greater number of them be excavations, ſome thouſands of miles deep, certain changes of the umbra would be obſervable when near the limb, as has been ſhewn at ſo much length. Were they very ſhallow, or quite ſuperficial, both ſides of the umbra would as to ſenſe contract alike in their progreſs toward the limb: for if in caſe 4th, above ſtated, the ſpot had been ſuppoſed ſuperficial, the apparent breadth of the ſide of the umbra next the center of the diſk, would have then been only 1″.62, and that of the ſide oppoſite 1″.27. Now, the whole of either of theſe quantities, and much more their difference, would be quite inſenſible. Again, if the nucleus extended much above the common level whilſt the ſurrounding umbra was ſuperficial, we ſhould behold manifeſt indications of this by ſuch an opaque body when ſeen very obliquely being projected acroſs the fartheſt ſide of the umbra, and by hiding the whole or part of it before the time it would otherways diſappear. According to this or that condition of the ſpot, ſuch things muſt infallibly obtain

by

by the known laws of viſion; and hence arguments reſting upon ſuch principles may be denominated *optical* ones. On the other hand, when ſpots are contemplated near the middle of the diſk, a great variety of changes are obſerved in them, which depend not upon poſition, but upon certain phyſical cauſes producing real alterations in their form and dimenſions. It is plain, that arguments derived from the conſideration of ſuch changes, and which, on that account, may be called *phyſical arguments*, can aſſiſt us but little in inveſtigating their third dimenſions; and, from the nature of the thing, muſt be liable to great uncertainty. The author of the Memoire, in p. 511. &c. takes new ground, and proceeds with a number of objections, depending upon that ſort of reaſoning which we have laſt defined. I muſt take notice, that a certain diſtinction has been here overlooked, which in my paper I endeavoured to point out. Preſuming upon our great ignorance of many things which doubtleſs affect deeply the conſtitution of that wonderful body the ſun, I offered, in part II. an account of the production changes and decay of the ſpots, conſidered as excavations, in the moſt looſe and problematical manner, ſtating every thing on this head in the form of queries. This account, crude and imperfect as it is, appeared to me much leſs incumbered with difficulties than any other, and of this ſome ſtriking examples are there ſet before the reader. But I have expreſsly owned, that many circumſtances ſtill remained unexplained; and upon the whole marked out the theory, if ſuch it may be called, as very imperfect. Nature unqueſtionably abounds with numberleſs unthought-of energies, and modes of working moſt curiouſly and moſt wiſely adapted to all ſituations in the material world: and in regard to that ſyſtem of economy which is eſtabliſhed in the ſun, producing

there ſuch a ſtrange fluctuation of appearances, human reaſon, even when aſpiring by the moſt enlarged analogy, muſt recoil under a conſciouſneſs of the unfathomable reſources of nature, and of its own dark and limited ſphere. "Demiraberis qui-"dem ſine dubio" (ſays HEVELIUS, ſpeaking of the Sun, Cometographia, p. 412.) "quod tam brevi tempore, ſpatio ali-"quot dierum, quin horarum, adeo miris et horribilibus ſub-"jiciantur mutationibus, ac viciſſitudinibus!"

Hence I would remark, that whatever inconſiſtencies are imagined in the account I have delivered in part II. though ſuch may be juſtly chargeable upon certain principles there aſſumed, yet they ought not to be ſtated as preſumptions againſt the ſpots being really excavations or depreſſions in the luminous matter of the ſun. This opinion muſt reſt entirely upon the evidence held forth in the firſt part of the paper, whatever be the fate of the account laid down in the ſecond. It does not enter there as an hypotheſis, but as a matter of fact, previouſly eſtabliſhed by *optical arguments*; and from optical arguments alone can there ariſe even any juſt preſumptions againſt it. The lameneſs of the views given in part II. may probably proceed, as we have ſaid, from our very imperfect knowledge of the vaſt range of phyſical cauſes which obtain in the univerſe. But whatever be their defects, no doubts ought to ariſe, upon ſuch grounds, of the ſpots being themſelves what direct obſervation declares them, namely, excavations in the ſun. Whether their firſt production and ſubſequent numberleſs changes depend upon the eructation of elaſtic vapour from below, or upon eddies or whirl-pools commencing at the ſurface, or upon the diſſolving of the luminous matter in the ſolar atmoſphere, as clouds are melted and again given out by our air; or, if the reader pleaſes, upon the annihilation

and reproduction of parts of this reſplendent covering; is left for theory to gueſs at. Though, however, many difficulties ſhould occur in an attempt of this kind, it would certainly be unreaſonable on that account to call in queſtion the third dimenſion of the ſpots, as previouſly determined by arguments which are liable to no fallacy, and which are unconnected with every kind of theoretical reaſoning.

Now, in the Memoire before me, this ſort of diſtinction has eſcaped the notice of the author. His optical arguments, indeed, as they regard the firſt part of my eſſay, put on a juſt and proper claim to be heared, and have now, as we conceive, been fully anſwered. But ſuperadded to theſe are many others, which, though they relate very properly to the view I have given in part II. and to that alone, yet finally are ſummed up along with the reſt, as not only militating ſtrongly againſt that particular view, but in order to diſprove that the ſpots are excavations in the luminous matter.

I here think it but juſtice to that honourable Body of Gentlemen who, in the year 1774, compoſed the Council of the Royal Society, or the Committee of Papers, to mention, that the publication of the ſecond part of mine was more owing to their having conſented to my requeſt, than to their own ſentiments in regard to the fitneſs of ſo doing. But as I had beſtowed ſome pains upon drawing up theſe views, and as care had alſo been taken to diſtinguiſh between fact and any thing like to theory, and as the latter was propounded only in the form of queries, there appeared to me no harm in letting that ſecond part go forth alſo; eſpecially as I flattered myſelf, that thereby a greater curioſity would have been excited, and the ſubject of courſe ſooner inquired into by obſervation.

As

As I conceive it, however, of ſome importa[illegible] to have the diſtinction above treated of perfectly underſtood in future, I now purpoſely avoid entering upon any theoretical ground whatever. My wiſh therefore is, that the author of the Memoire may acquit me of every thing not perfectly reſpectful, though I do not follow him through that train of objection founded upon vague and incompetent phyſical arguments, which is to be met with in p. 511. &c. By further conſidering the particulars hinted at in p. 21. and 29. of my paper, ſeveral difficulties, perhaps, may be removed; but we forbear any illuſtration of this kind, chiefly to evince how little we concern ourſelves whether the views delivered in part II. can ſtand of themſelves or not. Thoſe who do not like the principles there aſſumed, or the concluſions drawn from them; in ſhort, thoſe who will call part II. a theory, and who think it a bad one, may, if they pleaſe, mend it, or contrive a new and a better one of their own. But ſo long as they cannot, by irrefragable optical arguments, ſet aſide the induction laid down in part I. we muſt demand of them, ſo to fabricate their theories as to account for the various circumſtances of the ſpots, conſidered as things which poſſeſs three dimenſions, *viz.* length, breadth, and depth, or, in other words, as excavations in the luminous matter of the ſun.

This fact is the only one I am ſolicitous to maintain or to contend for; and for a very good reaſon, becauſe I conſider it as actually demonſtrated by competent obſervations. As ſuch, to indulge for a moment in a figure, it would be a pity not to reſcue it from being drawn into the eddy of ſome treacherous theory, the nature of all which is to ſweep into their vortex and finally to precipitate to the bottom every thing which obſtructs their impetuous career.

Sir ISAAC NEWTON, perceiving too well this proneneſs to ſyſtem, has laid down his fourth rule of philoſophizing, that arguments of induction may not be evaded by hypotheſis. It will become us, therefore, in all things, and in the preſent ſubject in particular, to have reſpect to ſo excellent a precept. In ſpeaking hereafter of the ſolar ſpots, let us ſeparate what things claim to be heard as matters of fact from what reſt upon the ſandy foundations of mere theory, and no longer confound them together.

Since upon this topic, I humbly beg the indulgence of the reader whilſt I advert to a certain abuſe of terms, which is but too prevalent in books of philoſophy, both in our own country and upon the continent. What I have to ſay relates to this word *hypotheſis*. "Quicquid enim non deducitur ex phæno-"menis hypotheſis vocanda eſt," are the words of Sir ISAAC NEWTON in his general ſcholium. And yet real diſcoveries, founded upon the beſt induction, are ſometimes mentioned by the appellation of ſuch and ſuch a one's hypotheſis. I have often thought, that this impropriety of language owes its continuance to the force of cuſtom, and that it is one of thoſe badges we ſtill retain of that diſgraceful ſtate philoſophy lay under before the æra of experiment and obſervation, when almoſt every thing was hypotheſis and theory both in name and in reality.

Moſt kind of hypotheſis regards true philoſophy with ſo unfriendly an aſpect, that we ſhould be careful at leaſt not to contaminate matter of fact and certain truth with ſo inauſpicious a denomination. I would alſo remark, that none which do not carry with them great marks of probability ſhould be brought into view, even in the way of hints or queries, for ſuggeſting further experiments and obſervations; and that far

leſs ought ſyſtems, built upon notions evidently incongruous, to have a place in any modern book of philoſophy. This has a tendency ſtill to favour that devious path, that *falſe taſte*, which it concerns philoſophy ſo much to guard againſt and to diſcourage.

It remains now only to make a few ſtrictures upon M. DE LA LANDE's theory of the ſolar ſpots, humbly ſubmitting them to the conſideration of the reader. The import of it is, "that the "ſpots as phenomena ariſe from dark bodies like rocks, which "by an alternate *flux* and *reflux* of the liquid igneous matter "of the ſun, ſometimes raiſe their heads above the general ſur-"face. That part of the opaque rock, which at any time thus "ſtands above, gives the appearance of the nucleus, whilſt "thoſe parts, which in each lie only a little under the igneous "matter, appear to us as the ſurrounding umbra."

In the firſt place it may be remarked, "that the whole "proceeds upon *mere ſuppoſition*." This, indeed, the author himſelf very readily acknowledges. Though, therefore, it could not be diſputed by arguments derived from obſervation, yet conjecture of any kind, if equally plauſible, might fitly be employed to ſet aſide its credit. Chooſing, however, to avoid a tedious diſcuſſion of this kind, or to try it upon the phenomena which are enumerated in p. 511. &c. by entering into arbitrary and diſputable principles, we ſhall confine ourſelves to ſuch particulars as appertain to the more obvious character of the ſpots, and which alſo ſeem to be irreconcileable with the theory; and firſt of all, in regard to the diſtinguiſhing features of the umbra.

M. CASSINI, Mem. Acad. tom. X. p. 582. plate 7. and M. DE LA HIRE, Mem. Acad. 1703, p. 16. and I may add all other obſervers, and all good repreſentations of the ſpots, bear teſti-

teſtimony to the exterior boundary of the umbra being always well defined, and to the umbra itſelf being leſs and leſs ſhady the nearer it comes to the nucleus. Now it may be aſked, how this could poſſibly be according to M. DE LA LANDE's theory? If the umbra be occaſioned by our ſeeing parts of the opaque rock, which lie a little under the ſurface of the igneous matter, ſhould it not always be darkeſt next the nucleus, and from the nucleus outward ſhould it not wax more and more bright, and at laſt loſe itſelf in the general luſtre of the ſun's ſurface, and not terminate all at once at the darkeſt ſhade, as in fact it does? Theſe few incongruities, which meet us as it were in the very threſhold of the theory, are ſo very palpable, that of themſelves they raiſe unſurmountable doubts. For, generally ſpeaking, the umbra immediately contiguous to the nucleus, inſtead of being very dark, as it ought to be, from our ſeeing the immerſed parts of the opaque rock through a thin ſtratum of the igneous matter, is on the contrary very nearly of the ſame ſplendour as the external ſurface.

Concerning the nucleus, or that part of the opaque rock which ſtands above the ſurface of the ſun, M. DE LA LANDE produces no optical arguments in ſupport of this third dimenſion or height. Neither does he ſay any thing particular as to the degree of elevation above the ſurface. But from what has been already hinted in the courſe of this paper, it appears, that if this were any thing ſenſible, it ought to be diſcovered by phenomena very oppoſite to thoſe which we have found to be ſo general.

Again, a flux and reflux of the igneous matter ſo conſiderable as ſometimes to produce a great number of ſpots all over the middle zone, might affect the apparent diameter of the ſun, making that which paſſes through his equator leſs than the

the polar one, by the retreat of the igneous matter towards those regions where no ſpots ever appear. But as a difference of this kind of nearly a thouſandth part of the whole would be perceivable, as we learn from M. DE LA LANDE's own obſervations, compared with thoſe of Mr. SHORT, in Hiſtoire Acad. 1760, p. 123. it would ſeem, that the theory had alſo this difficulty to combat. Further, when among ſpots very near one another, ſome are obſerved to be increaſing, whilſt others are diminiſhing, how is it poſſible that this can be the effect of ſuch a ſuppoſed flux and reflux? This laſt inconſiſtency is mentioned by the author himſelf, who endeavours to avoid it, by making a new demand upon the general fund of hypotheſis, deriving from thence ſuch qualities of the igneous matter as the caſe ſeems to require; and ſuch muſt be the method of proceeding in all ſyſtems merely theoretical.

But it is unneceſſary to purſue at more length illuſive ſpeculations of this kind, eſpecially as we lie under a conviction, founded upon fact, of the theory being utterly erroneous. It hardly differs in any reſpect from that propoſed by M. DE LA HIRE, and a little amended by the writer of the Hiſtoire de L'Acad. for 1707, p. 111. This near agreement, indeed, is taken notice of by M. DE LA LANDE himſelf, in his excellent aſtronomy.

The writer of the Hiſtoire de L'Acad. for 1719, p. 76. after reviewing the merits of this theory, and comparing it with ſeveral phenomena of the ſpots which had been obſerved for the four preceeding years, pronounces it unſatisfactory, and concludes his remarks with the following expreſſion: "Il ſera "plus naturel de croire qu'il ſe faſſe dans le ſoleil des "tions nouvelles, dependantes de quelque cauſe plus ou moins "forte ſelon les *circonſtances inconnues* ou elle ſe trouvera."

Views,

Views, much of the ſame kind, were even entertained by ſome ſo long ago as the days of SCHEINER, as we find mentioned by that indefatigable author in his Roſa Urſina, p. 746. "Non equidem me latebat," ſays he, "non deeſſe, qui putant "maculas ſolares eſſe quaſdam in ſole prominentias et *veluti* "*montes*; ſed cum hæc ex anticipata mentis affectione, "phænomenique ſolaris ignoratione procedant neglexi." And further on he adds: "Aſtronomi ſinceri eſt phænomenon ſequi "non antevertere: veritatem ex objecto accurate obſervato, "non objectum invitum ad arbitraria figmenta trahere."

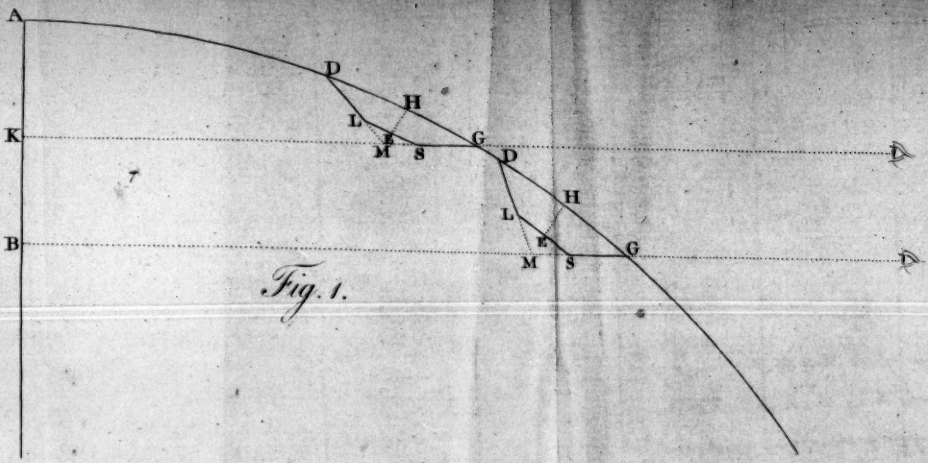


Fig. 1.

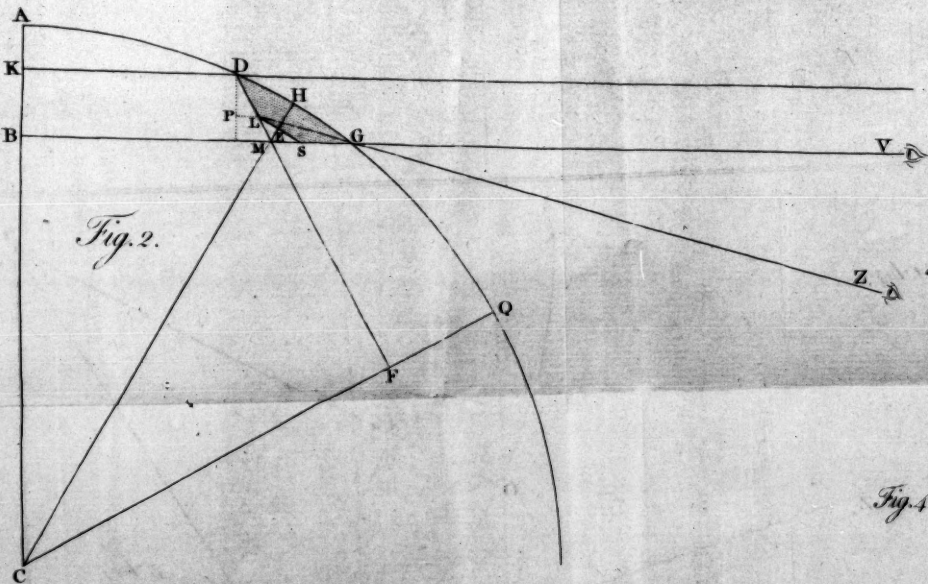


Fig. 2.

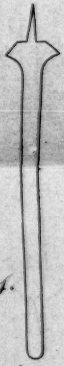
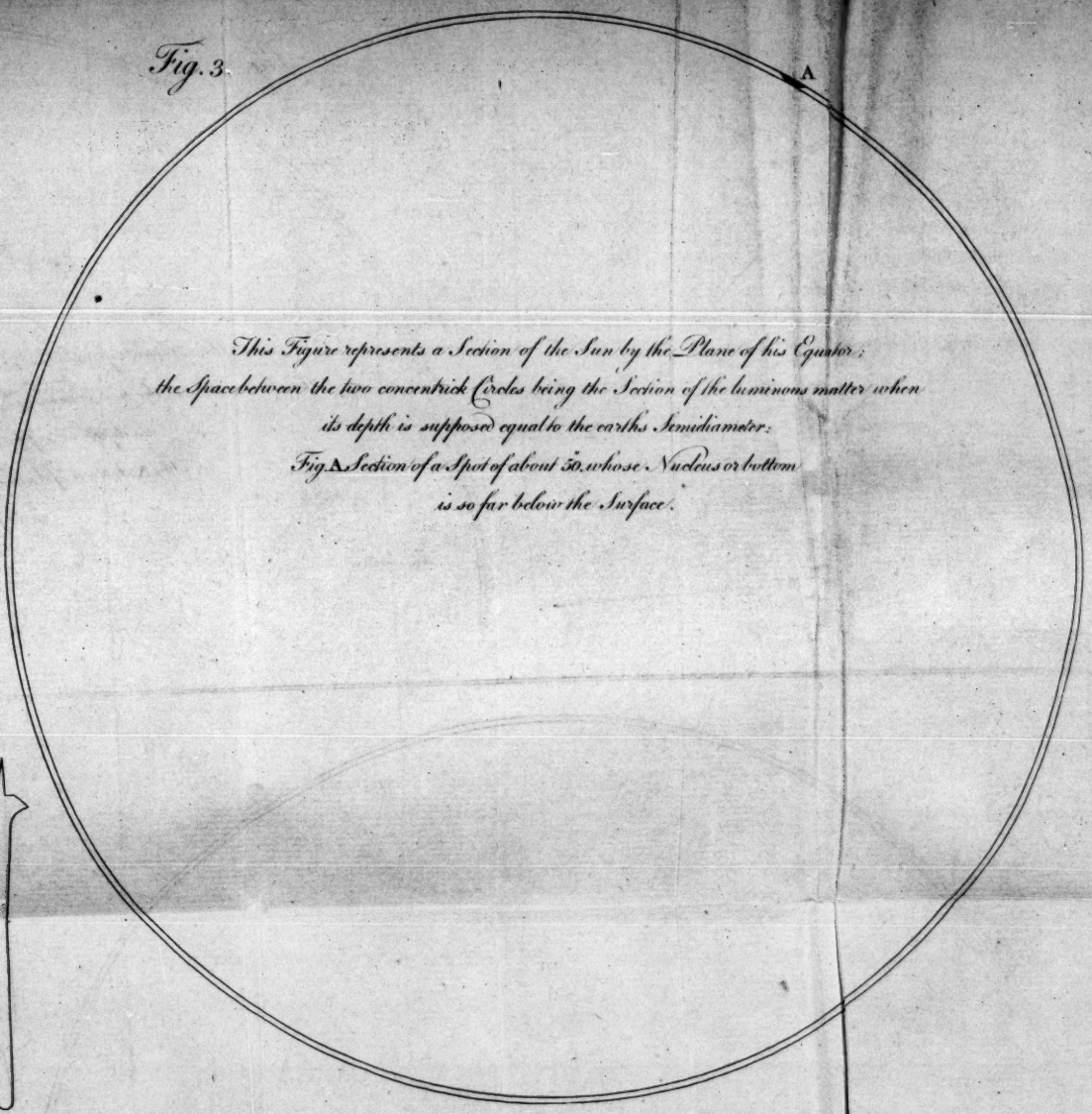


Fig. 4.

Fig. 3.



This Figure represents a Section of the Sun by the Plane of his Equator:
the Space between the two concentrick Circles being the Section of the luminous matter when
its depth is supposed equal to the earths Semidiameter:
Fig. A Section of a Spot of about 50. whose Nucleus or bottom
is so far below the Surface.

Basire Sc.

XI. *An Account of the Earthquakes which happened in* Italy, *from* February *to* May 1783. *By Sir* William Hamilton, *Knight of the Bath, F. R. S.; in a Letter to Sir* Joseph Banks, *Bart. P. R. S.*

Read July 3, 1783.

Naples, May 23, 1783.

I AM happy now to have it in my power to give you, and my brethren of the Royal Society, some little idea of the infinite damage done, and of the various phenomena exhibited, by the earthquakes (which began the 5th of February last, and continue to be sensibly, though less violently, felt to this day) in the two Calabrias, at Messina, and in the parts of Sicily nearest to the continent. From the most authentic reports, and accounts received at the offices of his Sicilian Majesty's secretary of state, we gathered in general, that the part of Calabria, which has been most affected by this heavy calamity, is that which is comprehended between the 38th and and 39th degree, that the greatest force of the earthquakes seemed to have exerted itself from the foot of those mountains of the Apennines called the Monte Deio, Monte Sacro, and Monte Caulone, extending westward to the Tyrrene sea; that the towns, villages, and farm-houses, nearest these mountains, situated either on hills or in the plain, were totally ruined by the first shock of the 5th of February about noon; and that the

greateſt mortality was there; that in proportion as the towns and villages were at a greater diſtance from this center, the damage they received was leſs conſiderable; but that even thoſe more diſtant towns had been greatly damaged by the ſubſequent ſhocks of the earthquake, and eſpecially by thoſe of the 7th, the 26th, and 28th of February, and that of the 1ſt of March; that from the firſt ſhock, the 5th of February, the earth continued to be in a continual tremor, more or leſs; and that the ſhocks were more ſenſibly felt at times in ſome parts of the afflicted provinces than in others; that the motion of the earth had been various, and, according to the Italian denomination, *vorticoſo*, *orizontale*, and *oſcillatorio*, either whirling like a vortex horizontal, or by pulſations, or beatings from the bottom upwards; that this variety of motion had increaſed the apprehenſions of the unfortunate inhabitants of thoſe parts, who expected every moment that the earth would open under their feet, and ſwallow them up; that the rains had been continual and violent, often accompained with lightning and irregular and furious guſts of wind; that from all theſe cauſes the face of the earth of that part of Calabria (comprehended as abovementioned between the 38th and 39th degrees) was entirely altered, particularly on the weſtward ſide of the mountains above named; that many openings and cracks had been made in thoſe parts; that ſome hills had been lowered, and others quite levelled; that in the plains, deep chaſms had been made, by which many roads were rendered impaſſable; that huge mountains had been ſplit aſunder, and parts of them driven to a conſiderable diſtance; that deep vallies had been filled up by the mountains (which formed thoſe vallies) having been detached by the violence of the earthquakes, and joined together; that the courſe of ſome rivers had been altered;

that many ſprings of water had appeared in places that were perfectly dry before; and that in other parts, ſprings that had been conſtant had totally diſappeared; that near Laureana in Calabria Ultra, a ſingular phenomenon had been produced, that the ſurface of two whole tenements, with large olive and mulberry-trees therein, ſituated in a valley perfectly level, had been detached by the earthquake, and tranſplanted, the trees ſtill remaining in their places, to the diſtance of about a mile from their firſt ſituations; and that from the ſpot on which they formerly ſtood hot water had ſprung up to a conſiderable height, mixed with ſand of a ferruginous nature; that near this place alſo ſome countrymen and ſhepherds had been ſwallowed up with their teams of oxen and their flocks of goats and ſheep; in ſhort, that beginning from the city of Amantea, ſituated on the coaſt of the Tyrrene ſea in Calabria Citra, and going along the weſtward coaſt to Cape Spartivento in Calabria Ultra, and then up the eaſtern coaſt as far as the Cape d'Alice (a part of Calabria Citra on the Ionian ſea), there is not a town or village, either on the coaſt or land, but what is either totally deſtroyed, or has ſuffered more or leſs, amounting in all to near four hundred, what are called here Paeſes; a village containing leſs than an hundred inhabitants is not counted as a Paeſe.

The greateſt mortality fell upon thoſe towns and countries ſituated in the plain on the weſtern ſide of the mountains Dejo, Sacro, and Caulone. At Caſal Nuovo, the Princeſs Gerace, and upwards of 4000 of the inhabitants, loſt their lives; at Bagnara, the number of dead amounts to 3017; Radicina and Palmi count their loſs at about 3000 each; Terranuova about 1400; Seminari ſtill more. The ſum total of the mortality in both Calabrias and in Sicily, by the earthquakes

alone, according to the returns in the ſecretary of ſtate's office at Naples, is 32,367; but I have good reaſon to believe that, including ſtrangers, the number of lives loſt muſt have been conſiderably greater, 40,000 at leaſt may be allowed, and, I believe, without any exaggeration.

From the ſame office intelligence we likewiſe heard, that the inhabitants of Scilla on the firſt ſhock of the earthquake, the 5th of February, had eſcaped from their houſes on the rock, and, following the example of their prince, taken ſhelter on the ſea-ſhore; but that in the night-time the ſame ſhock, which had raiſed and agitated the ſea ſo violently, and done ſo much damage on the point of the Faro of Meſſina, had acted with ſtill greater violence there, for that the wave (which was repreſented to have been boiling hot, and that many people had been ſcalded by its riſing to a great height) went furiouſly three miles inland, and ſwept off in its return 2473 of the inhabitants of Scilla, with the prince at their head, who were at that time either on the Scilla Strand, or in boats near the ſhore.

All accounts agreed, that of the number of ſhocks which have been felt ſince the beginning of this formidable earthquake, amounting to ſome hundreds, the moſt violent, and of the longeſt duration, were thoſe of the 5th of February at 19½ (according to the Italian way of counting the hours); of the 6th of February, at 7 hours in the night; of the 27th of February, at 11¼ in the morning; of the firſt of March, at 8¼ in the night; and that of the 28th of March, at 1¼ in the night. It was this laſt ſhock that affected moſt the upper part of Calabria Ultra, and the lower part of the Citra, an authentic deſcription of which you will ſee hereafter, in a letter which I received from the Marquis Ippolito, an accurate obſerver reſiding

residing at Catanzaro in the upper Calabria. The first and the last shocks must have been tremendous indeed, and only these two were sensibly felt in this capital.

The accounts which this government has received from the province of Cosenza, are less melancholy than those from the province of Calabria Ultra. From Cape Suvero to the Cape of Cetraro on the western coast, the inland countries, as well as those on the coast, are said to have suffered more or less in proportion to their proximity to the supposed center of the earthquakes; and it has been constantly observed, that its greatest violence has been exerted, and still continues to be so, on the western side of the Appennines, precisely the celebrated Sila of the ancient Brutii, and that all those countries situated to the eastward of the Sila had felt the shocks of the earthquake, but without having received any damage from them. In the province of Cosenza there does not appear to be above 100 lives lost. In the last accounts from the most afflicted part of Calabria Ultra, two singular phenomena are mentioned. At about the distance of three miles from the ruined city of Oppido, there was a hill (the soil of which is a sandy clay) about 500 palms high, and 1300 in circumference at its basis. It was said, that this hill, by the shock of the 5th of February, jumped to the distance of about four miles from the spot where it stood into a plain called the Campo di Bassano. At the same time the hill on which the town of Oppido stood, which extended about three miles, divided in two, and as its situation was between two rivers, its ruins filled up the valley, and stopped the course of those rivers; two great lakes are already formed, and are daily increasing, which lakes, if means are not found to drain them, and give the rivers their due course, in a short time must infect the air greatly.

From

From Sicily the accounts of the moſt ſerious nature were thoſe of the deſtruction of the greateſt part of the noble city of Meſſina, by the ſhock of the 5th of February, and of the remaining parts by the ſubſequent ones;—that the kay in the port had ſunk conſiderably, and was in ſome places a palm and a half under water;—that the ſuperb building, called the Palazzata, which gave the port a more magnificent appearance than any port in Europe can boaſt of, had been entirely ruined;—that the Lazaret had been greatly damaged; but that the citadel had ſuffered little;—that the mother church had fallen; in ſhort, that Meſſina was no more;—that the tower at the point of the entrance of the Faro was half deſtroyed; and that the ſame hot wave, that had done ſuch miſchief at Scilla, had paſſed over the point of land at the Faro, and carried off about 24 people. The viceroy of Sicily likewiſe gave an account of ſome damage done by the earthquakes, but nothing conſiderable, at Melazzo, Patti, Terra di Santa Lucia, Caſtro Reale, and in the iſland of Lipari.

This, Sir, was the intelligence I was poſſeſſed of the end of laſt month; but as I am particularly curious, as you know, on the ſubject of volcanoes, and was perſuaded in my own mind (from the preſent earthquakes being confined to one ſpot) that ſome great chemical operation of nature of the volcanic ſort was the real cauſe of them; in order to clear up many points, and to come at truth, which you alſo well know, Sir, is exceedingly difficult, I took the ſudden reſolution to employ about twenty days (which was as much as I could allow, and have time to be out of Italy, in my way home, before the heats ſet in) in making the tour of ſuch parts of Calabria Ultra and Sicily as had been, and were ſtill, moſt affected by the earthquakes, and examining with my own eyes the phenomena above mentioned. I accordingly

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hired for that purpoſe a Malteſe Speronara for myſelf, and a Neapolitan Felucca for my ſervants, and left Naples the 2d of May. I was furniſhed, by command of his Sicilian Majeſty, with ample paſſports, and orders to the commanding officers of the different provinces to give me every aſſiſtance and protection in the purſuit of my object. I had a pleaſant voyage in my Malteſe Speronara (which are excellent boats, and the boatmen very ſkilful) along the Coaſt of the Principato Citra and Calabria Citra, after having paſſed the gulph of Policaſtro. At Cedraro I found the firſt ſymptoms of the earthquake, ſome of the principal inhabitants of that city having quitted their houſes, and living in new erected barracks, though not a houſe in the whole town, as I could ſee, had ſuffered. At St. Lucido I perceived that the baron's palace and the church ſteeple had ſuffered, and that moſt of the inhabitants were in barracks. The barracks are juſt ſuch ſort of buildings as the booths of our country fairs, though indeed many I have ſeen are more like our pig-ſtyes. As my object was to get as faſt as poſſible to the center of the miſchief, having little time, and much to ſee, I contented myſelf with a diſtant view of Maida, Nicaſtro, and Santo Eufemia, and puſhed on to the town of Pizzo in Calabria Ultra, where I landed on the evening of the 6th of May. This town, ſituated on the ſea, and on a volcanic tuffa *, had been greatly damaged by the earthquake of the 5th of February, but was completely ruined by that of the 28th of March. As the inhabitants of this town (amounting to about 5000) had ſufficient warning, and had left their houſes, and taken to barracks on the firſt ſhock the 5th of February, the mortality on the 28th of

* This was the only token of former volcanic explosions that I met with in Calabria.

March

March was inconſiderable; but, from the barracks having been ill-conſtructed, and many ſituated in a very confined unwholeſome ſpot, an epidemical diſorder had taken place, and carried off many, and was ſtill in fatal force whilſt I was there, in ſpite of the wiſe endeavours of government to ſtop its progreſs. I fear, as the heats increaſe, the ſame misfortune will attend many parts of the unfortunate Calabria, as alſo the city of Meſſina. The inhabitants of Pizzo ſeemed to me to have habituated themſelves already to their preſent inconvenient manner of living, and ſhops of every kind were opened in the ſtreets of the barracks, which, except ſome few, are but poorly conſtructed. I was aſſured here, that the volcano of Stromboli, which is oppoſite, and in full view of, this town, and at the diſtance of about fifty miles, had ſmoked leſs, and thrown up a leſs quantity of inflamed matter during the earthquakes than it had done for ſome years paſt; that ſlight ſhocks continued to be felt daily; and the night I ſlept here, on board the Speronara drawn on ſhore, I was awakened with a ſmart one, which ſeemed to lift up the bottom of the boat, but it was not attended with any ſubterraneous noiſe. My ſervants, in the other boat, felt the ſame. The next day I ordered my boats to proceed to Reggio, and I went on horſeback to Monteleone, about ſix miles from Pizzo, up hill, on a road of looſe ſtones and clay, ſcarcely paſſable in this ſeaſon, but through the moſt beautiful and fertile country I ever beheld: a perfect garden of olive-trees, mulberry-trees, fruit-trees, and vines; and under theſe trees the richeſt crops of corn or lupins, beans or other vegetables, which ſeemed to thrive perfectly, though under a thick ſhade. This is the ſtile of the whole plain of Monteleone, except that here and there are vaſt woods of oak and olive-trees mixed, and the olive-

trees of ſuch a ſize as I could never have conceived, being half as big as the oaks themſelves, which are fine timber-trees, and more than treble the ſize of the olive-trees of the Campagna Felice. The olive woods, in ſome parts of the plain, are regularly planted in lines, and in others grow irregularly. Though the object of my preſent journey was merely to take a haſty view of the ſpots which had ſuffered ſo much by the calamity, my attention was continually called away, and I was loſt in the admiration of the fertility and beauty of this rich province, exceeding by many degrees (as to the firſt point) every country I have yet ſeen. Beſides the two rich products of ſilk and oil, in which this province ſurpaſſes every other, perhaps, in the whole world, it abounds with corn, wine, cotton, liquorice, fruit, and vegetables of every kind; and if its population and induſtry kept pace with its fertility, the revenue of Calabria Ultra might ſurely be more than doubled in a ſhort time. I ſaw whole groves of mulberry-trees, the owners of which told me, did not let for more than five ſhillings an acre, when every acre would be worth at leaſt five pounds, had they hands to gather the leaves and attend the ſilk-worms. The town of Monteleone, anciently Vibo Valentia, is beautifully ſituated on a hill, overlooking the ſea and the rich plains above mentioned, bounded by the Apennines, and crowned by Aſpramonte, the higheſt of them all, interſperſed with towns and villages, which, alas! are no more than heaps of ruins. The town of Monteleone ſuffered little by the firſt ſhocks of the earthquake; but was greatly damaged by that of the 28th of March (though only twelve lives were loſt), and all the inhabitants are reduced to live in barracks, many of which are well conſtructed with either planks or reeds, covered with plaiſter on the outſide. As

this country has ever been ſubject to earthquakes, the barons had uſually a barrack near their palace, to retire to on the leaſt alarm of an earthquake. I inhabited here a magnificent one, conſiſting of many rooms well furniſhed, which was built by the preſent Duke of Monteleone's grand-father. I owe the ſafety and the expedition of the very intereſting journey which I have taken through this province to this duke's goodneſs, as he was pleaſed at Naples to furniſh me with a letter to his agent; in conſequence of which, I was not only moſt hoſpitably and elegantly treated in his barrack, and ſupplied with excellent ſure-footed horſes for myſelf and ſervant, but alſo with two of his horſe-guards, well acquainted with the croſs roads of the country, without which it would have been impoſſible, with any degree of ſafety, to have viſited every curious ſpot between Monteleone and Reggio, as I did, in four days. No one, that has not had the experience, can conceive the horrid ſtate of the roads in Calabria, even in this ſeaſon, nor the ſuperior excellence of the horſes of the country. All agreed here that every ſhock of the earthquake ſeemed to come with a rumbling noiſe from the weſtward, beginning uſually with the horizontal motion, and ending with the vorticoſe, which is the motion that has ruined moſt of the buildings in this province. The ſame obſervation I found to be a general one throughout this province. I found it a general obſervation alſo, that before a ſhock of an earthquake, the clouds ſeemed to be fixed and motionleſs; and that immediately after a heavy ſhower of rain, a ſhock quickly followed. I ſpoke with many here and elſewhere, who were thrown down by the violence of ſome of the ſhocks; and ſeveral peaſants in the country told me, that the motion of the earth was ſo violent, that the heads

heads of the largeſt trees almoſt touched the ground from ſide to ſide; that during a ſhock, oxen and horſes extended their legs wide aſunder not to be thrown down, and that they gave evident ſigns of being ſenſible of the approach of each ſhock. I myſelf obſerved, that in the parts that have ſuffered moſt by the earthquakes, the braying of an aſs, the neighing of a horſe, or the cackling of a gooſe, always drove people out of their barracks, and was the occaſion of many pater-noſters and ave-marias being repeated in expectation of a ſhock. From Monteleone I deſcended into the plain, having paſſed through many towns and villages which had been more or leſs ruined according to their vicinity to the plain. The town of Mileto, ſituated in a bottom, I ſaw was totally deſtroyed, and not a houſe ſtanding. At ſome diſtance I ſaw Soriano and the noble Dominican convent a heap of ruins; but as my object was not to viſit ruins, but the greater phenomena produced by the earthquakes, I went on to Roſarno. I muſt, however, firſt mention the moſt remarkable inſtance I met with of animals being able to live long without food, of which there have been many examples during theſe preſent earthquakes. At Soriano two fattened hogs, that had remained buried under a heap of ruins, were taken out alive the forty-ſecond day; they were lean and weak, but ſoon recovered. One of his Sicilian majeſty's engineers, who was preſent at the taking them out, gave me this information. It was evident to me, in this day's journey, that all habitations ſituated on high grounds, the ſoil of which is a gritty ſand ſtone, ſomewhat like a granite, but without the conſiſtence, had ſuffered leſs than thoſe ſituated in the plain, which are univerſally levelled to the ground. The ſoil of the plain is a ſandy clay, white, red, or brown; but

the white prevails moſt, and is full of marine ſhells, particularly ſcollop ſhells. This valley of clay is interſected in many parts by rivers and torrents coming from the mountains, which have produced wide and deep ravines all over the country. Soon after we had paſſed through the ruined town of St. Pietro, we had a diſtant view of Sicily, and the ſummit of Mount Etna, which ſmoked conſiderably. Juſt before we arrived at Roſarno, near a ford of the river Mamella we paſſed over a ſwampy plain, in many parts of which I was ſhewn ſmall hollows in the earth, of the ſhape of an inverted cone: they were covered with ſand, as was the ſoil near them. I was told that, during the earthquake of the 5th of February, from each of theſe ſpots a fountain of water mixed with ſand had been driven up to a conſiderable height. I ſpoke to a peaſant here, who was preſent, and was covered with the water and ſand; but aſſured me, that it was not hot, as had been repreſented. Before this appearance, he ſaid, the river was dry; but ſoon after returned and overflowed its banks. I afterwards found, that the ſame phenomenon had been conſtant with reſpect to all the other rivers in the plain during the formidable ſhock of the 5th of February. I think this phenomenon is eaſily explained, by ſuppoſing the firſt impulſe of the earthquake to have come from the bottom upwards, which all the inhabitants of the plain atteſt to be fact; the ſurface of the plain ſuddenly riſing, the rivers, which are not deep, would naturally diſappear, and the plain, returning with violence to its former level, the rivers muſt naturally have returned, and overflowed, at the ſame time that the ſudden depreſſion of the boggy grounds would as naturally force out the water that lay hid under their ſurface. I obſerved in the other parts where this ſort of phenomenon had been exhibited, that

the ground was always low and ruſhy. Between this place and Roſarno we paſſed the river Meſſano or Metauro (which is near the town above mentioned) on a ſtrong timber bridge, 700 palm long, which had been lately built by the Duke of Montelcone. From the cracks made on the banks and in the bed of the river by the earthquake, it was quite ſeparated in one part, and the level on which the piers were placed having been variouſly altered, the bridge has taken an undulated form, and the rail on each ſide is curiouſly ſcolloped; but the parts that were ſeparated having been joined again, it is now paſſable. The duke's bridge-man told me alſo, that at the moment of the earthquake, this great river was perfectly dry for ſome ſeconds, and then returned with violence, and overflowed; and that the bridge undulated in a moſt extraordinary manner. When I mention the earthquake in the plain, it muſt be always underſtood the firſt ſhock of the 5th of February, which was by far the moſt terrible, and was the one that did the whole miſchief in the plain, without having given any previous notice. The town of Roſarno, with the Duke of Monteleone's palace there, was entirely ruined; but the walls remained about ſix feet high, and are now fitting up as barracks. The mortality here did not much exceed 200 out of near 3000. It had been remarked at Roſarno, and the ſame remark has been conſtantly repeated to me in every ruined town that I have viſited, that the male dead were generally found under the ruins in the attitude of ſtruggling againſt the danger; but that the female attitude was uſually with hands claſped over their heads, as giving themſelves up to deſpair, unleſs they had children near them; in which caſe they were always found claſping the children in their arms, or in ſome attitude which indicated their anxious care to protect them; a ſtrong inſtance

of

of the maternal tenderneſs of the ſex! The only building that remained unhurt at Roſarno was a ſtrong built town gaol, in which were three notorious villains, who would probably have loſt their lives had they been at liberty. After having dined in a barrack, the owner of which had loſt five of his family by the earthquake, I proceeded to Laureana, often croſſing the wide extended bed of the river Metauro. The environs of Laureana, which ſtands on an elevation, is the garden of Eden itſelf; nothing I ever ſaw can be compared to it. The town is conſiderable; but as the earthquake did not come on ſuddenly, as in the plain, not a life was loſt there; but from a ſickneſs, occaſioned by hardſhips and fright, 52 have died ſince. I lodged in the barracks of a ſenſible gentleman of Mileto, Don Domenico Acquanetta, who is a principal proprietor of this town. He attended me the next day to the two tenements, called the Macini and Vaticano, mentioned in the former part of this letter, and which were ſaid to have changed their ſituation by the earthquake. The fact is true, and eaſily accounted for. Theſe tenements were ſituated in a valley ſurrounded by high grounds, and the ſurface of the earth, which has been removed, had been probably long undermined by little rivulets, which come from the mountains, and now are in full view on the bare ſpot the tenements had deſerted. Theſe rivulets have a ſufficiently rapid courſe down the valley, to prove its not being a perfect level as was repreſented. I ſuppoſe the earthquake to have opened ſome depoſitions of rain-water in the clay hills which ſurround the valley, which water, mixed with the looſe ſoil, taking its courſe ſuddenly through the undermined ſurface, lifting it up with the large olive and mulberry-trees, and a thatched cottage, floated the entire piece of ground, with all its vegetation, about a mile down

down the valley, where it now ſtands, with moſt of the trees erect. Theſe two tenements may be about a mile long, and half a mile broad. I was ſhewn ſeveral deep cracks in this neighbourhood, not one above a foot in breadth; but which, I was credibly aſſured, had opened wide during the earthquake, and ſwallowed up an ox, and near an hundred goats, but no countrymen, as was reported. In the valley above mentioned I ſaw the ſame ſort of hollows in the form of inverted cones, out of which, I was aſſured, that hot water and ſand had been emitted with violence during the earthquakes as at Roſarno; but I could not find any one who could poſitively affirm that the water had been really hot, although the reports which government received affirm it. Some of the ſand thrown out here with the water has a ferruginous appearance, and ſeems to have been acted upon by fire. I was told, that it had alſo, when freſh, a ſtrong ſmell of ſulphur, but I could not perceive it.

From hence I went through the ſame delightful country to the town of Poliſtene. To paſs through ſo rich a country, and not ſee a ſingle houſe ſtanding on it, is moſt melancholy indeed; wherever a houſe ſtood, there you ſee a heap of ruins, and a poor barrack, with two or three miſerable mourning figures ſitting at the door, and here and there a maimed man, woman, or child, crawling upon crutches. Inſtead of a town, you ſee a confuſed heap of ruins, and round about them number of poor huts or barracks, and a larger one to ſerve as a church, with the church bells hanging upon a ſort of low gibbet; every inhabitant with a doleful countenance, and wearing ſome token of having loſt a parent.

I travelled four days in the plain, in the midſt of ſuch miſery as cannot be deſcribed. The force of the earthquake was ſo great there, that all the inhabitants of the towns were buried

either alive or dead under the ruins of their houſes in an inſtant. The town of Poliſtene was large, but ill ſituated between two rivers, ſubject to overflow. 2100 out of about 6000 loſt their lives here the fatal 5th of February. The Marquis St. Giorgio, the baron of this country, whom I found here, was well employed in aſſiſting his tenants. He had cauſed the ſtreets of his ruined town to be cleared of rubbiſh, and had erected barracks on a healthy ſpot near it, for the remainder of his ſubjects, and on a good plan. He had alſo conſtructed barracks of a larger ſize for the ſilk-worms, which I found already at work in them. This prince's activity and generoſity is moſt praiſe-worthy, and, as far as I have ſeen hitherto, he is without a rival. I obſerved, that the town of St. Giorgio, on a hill about two miles from Poliſtene, though rendered uninhabitable, was by no means levelled like the towns in the plain. There was a nunnery at Poliſtene; being curious to ſee the nuns that had eſcaped, I aſked the marquis to ſhew me their barracks; but, it ſeems, only one out of twenty-three had been dug out of her cell alive, and ſhe was fourſcore years of age. After having dined with the marquis in his humble barrack, near the ruins of his very magnificent palace, I went through a fine wood of olive, and another of cheſnut trees, to Caſal Nuovo, and was ſhewn the ſpot on which ſtood the houſe of my unfortunate friend the princeſs Gerace Grimaldi, who with more than four thouſand of her ſubjects loſt her life by the ſudden exploſion of the 5th of February (for ſo it appears to have been) that reduced this town to atoms. I was told by ſome here, who had been dug out of the ruins, that they felt their houſes fairly lifted up, without having had the leaſt previous notice. In other towns ſome walls and parts of houſes are ſtanding; but here you neither diſtinguiſh ſtreet or houſe,

houſe, all lye in one confuſed heap of ruins. An inhabitant of Caſal Nuovo told me, he was on a hill at the moment of the earthquake, overlooking the plain, when feeling the ſhock, and turning towards the plain, inſtead of the town, he ſaw in the place of it a thick cloud of white duſt like ſmoke, the natural effect of the cruſhing of the buildings, and the mortar flying off.

From hence I went through the towns of Caſtellace and Milicuſco (both in the ſame condition as Caſal Nuovo) to Terra Nuova, ſituated in the ſame lovely plain, between two rivers, which, with the torrents from the mountains, have, in the courſe of ages, cut deep and wide chaſms in the ſoft ſandy clay ſoil of which the whole plain is compoſed. At Terra Nuova the ravine or chaſm is not leſs than 500 feet deep, and three quarters of a mile broad. What cauſes a confuſion in all the accounts of the phenomena produced by this earthquake in the plain, is the not having ſufficiently explained the nature of the ſoil and ſituation. They tell you, that a town has been thrown a mile from the place where it ſtood, without mentioning a word of a ravine; that woods and corn-fields had been removed in the ſame manner, when in truth it is but upon a large ſcale, what we ſee every day upon a ſmaller, when pieces of the ſides of hollow ways, having been undermined by rain waters, are detached into the bottom by their own weight. Here, from the great depth of the ravine, and the violent motion of the earth, two huge portions of the earth, on which a great part of the town ſtood, conſiſting of ſome hundreds of houſes, were detached into the ravine, and nearly acroſs it, about half a mile from the place where they ſtood; and what is moſt extraordinary, ſeveral of the inhabitants of thoſe houſes, who had taken this ſingular leap in them, were nevertheleſs dug out alive, and ſome unhurt. I ſpoke to one myſelf who had taken this extrordinary journey in his houſe, with his wife and a

maid-ſervant: neither he nor his maid-ſervant were hurt; but he told me, his wife had been a little hurt, but was now nearly recovered. I happened to aſk him, what hurt his wife had received? His anſwer, though of a very ſerious nature, will nevertheleſs, I am ſure, make you ſmile, Sir, as it did me. He ſaid, ſhe had both her legs and one arm broken, and that ſhe had a fracture on her ſkull ſo that the brain was viſible. It appears to me, that the Calabreſi have more firmneſs than the Neapolitans; and they really ſeem to bear their exceſſive preſent misfortune with a true philoſophic patience. Of 1600 inhabitants at Terra Nuova, only 400 eſcaped alive. My guide there, who was a prieſt and phyſician, had been ſhut up in the ruins of his houſe by the firſt ſhock of the earthquake, and was blown out of it, and delivered by the ſucceeding ſhock, which followed the firſt immediately. There are many well-atteſted inſtances of the ſame having happened elſewhere in Calabria. In other parts of the plain ſituated near the ravine, and near the town of Terra Nuova, I ſaw many acres of land with trees and corn-fields that had been detached into the ravine, and often without having been overturned, ſo that the trees and crops were growing as well as if they had been planted there. Other ſuch pieces were lying in the bottom, in an inclined ſituation; and others again that had been quite overturned. In one place, two of theſe immenſe pieces of land having been detached oppoſite to one another, had filled the valley, and ſtopped the courſe of the river, the waters of which were forming a great lake: and this is the true ſtate of what the accounts mention of mountains that had walked, and joined together, ſtopped the courſe of the river, and formed a lake. At the moment of the earthquake the river diſappeared here, as at Roſarno, and returning ſoon after, overflowed the

bottom of the ravine about three feet in depth, ſo that the poor people that had been thrown with their houſes into the ravine from the top of it, and had eſcaped with broken bones, were now in danger of being drowned. I was aſſured, that the water was ſalt, like that of the ſea; but this circumſtance ſeems to want confirmation. The ſame reaſon I have given for the ſudden diſappearing of the river Metauro at Roſarno will account for the like phenomenon here, and in every part of the country where the rivers dried up at the moment of the earthquake. The whole town of Mollochi di Sotto near Terra Nuova, was likewiſe detached into the ravine, and a vineyard of many acres near it lies in the bottom of the ravine as I ſaw in a perfect order, but in an inclined ſituation: there is a foot-path through this vineyard, which has a ſingular effect, conſidering its preſent impracticable ſituation. Some water mills, that were on the river, having been jammed between two ſuch detached pieces as above deſcribed, were lifted up by them, and are now ſeen on an elevated ſituation, many feet above the level of the river. Without the proper explanations it is no wonder that ſuch facts ſhould appear miraculous. I obſerved in ſeveral parts of the plain, that the ſoil with timber trees and crops of corn, conſiſting of many acres, had ſunk eight and ten feet below the level of the plain; and in others again I perceived it had riſen as many. It is neceſſary to remember, that the ſoil of the plain is a clay mixed with ſand, which is eaſily moulded into any ſhape. In the plain, near the ſpots from whence the above mentioned pieces had been detached into the ravine, there were ſeveral parallel cracks, ſo that had the violence of the ſhocks of the earthquake continued, theſe pieces alſo would have probably followed. I remarked conſtantly in all my journey, that near every ravine, or hollow way, the parts of the plain adjoining

were full of large parallel cracks. The earth rocking with violence from ſide to ſide, and having a ſupport on one ſide only, accounts well for this circumſtance. From Terra Nuova I went to Oppido. This city is ſituated on a mountain of a ferruginous ſort of gritty ſtone, unlike the clay ſoil of its neighbourhood, and is ſurrounded by two rivers in a ravine deeper and broader than that of Terra Nuova. Inſtead of the mountain on which Oppido was ſituated having ſplit in two, and by its fall on the rivers, ſtopped their courſe and formed great lakes, as we were told; it was (as at Terra Nuova) huge pieces of the plain on the edge of the ravine, that had been detached into it, nearly filled it up, and ſtopped the courſe of the rivers, the waters of which are now forming two great lakes. It is true, that part of the rock on which Oppido ſtood was detached with ſeveral houſes into the ravine; but that is a trifling circumſtance in compariſon of the very great tracts of land, with large plantations of vines and olive-trees, which have been detached from one ſide of the ravine clear over to the other, though the diſtance is more than half a mile. It is well atteſted, that a countryman, who was ploughing his field in this neighbourhood with a pair of oxen, was tranſported with his field and team clear from one ſide of a ravine to the other, and that neither he nor his oxen were hurt. After what I have ſeen, I verily believe this may have happened. A large volume might be compoſed of the curious facts and accidents of this kind produced by the earthquakes in the valley; and, I ſuppoſe, many will be recorded in the account of the late formidable earthquakes, which the Academy of Naples intend to publiſh, the preſident having already ſent into Calabria fifteen members, with draughtſmen in proportion, to collect the facts, and make drawings for the ſole purpoſe of

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giving a ſatisfactory and ample account of the late calamity to the publick; but unleſs they attend, as I did, to the nature of the ſoil of the local where thoſe accidents happened, their reports will generally meet with little credit, except from thoſe who are profeſſed dilettanti of miracles, and many ſuch do certainly exiſt in this country. I met with a remarkable inſtance here of the degree of immediate diſtreſs to which the unfortunate inhabitants of the deſtroyed towns were reduced. Don Marcello Grillo, a gentleman of fortune, and of great landed property, having eſcaped from his houſe at Oppido, which was deſtroyed by the earthquake, and his money (no leſs than twelve thouſand pieces of gold) having been buried under the ruins of it, remained ſeveral days without food or ſhelter during heavy rains, and was obliged to a hermit in the neighbourhood for the loan of a clean ſhirt. Having walked over the ruins of Oppido, I deſcended into the ravine, and examined carefully the whole of it. Here I ſaw, indeed, the wonderful force of the earthquake, which has produced exactly the ſame effects as I have deſcribed in the ravine of Terra Nuova, but on a ſcale infinitely greater. The enormous maſſes of the plain, detached from each ſide of the ravine, lye ſometimes in confuſed heaps, forming real mountains, and having ſtopped the courſe of two rivers (one of which is very conſiderable) great lakes are already formed, and, if not aſſiſted by nature or art, ſo as to give the rivers their due courſe, muſt infallibly be the cauſe of a general infection in the neighbourhood. Sometimes I met with a detached piece of the ſurface of the plain (of many acres in extent) with the large oaks and olive-trees, with lupins or corn under them, growing as well, and in as good order at the bottom of the ravine, as their companions, from whom they were ſeparated, do on their native

ſoil

ſoil in the plain, at leaſt 500 feet higher, and at the diſtance of about three quarters of a mile. I met with whole vineyards in the ſame order in the bottom, that had likewiſe taken the ſame journey. As the banks of the ravine, from whence theſe pieces came, are now bare and perpendicular, I perceived that the upper ſoil was a reddiſh earth, and the under one a ſandy white clay, very compact, and like a ſoft ſtone; the impulſe theſe huge maſſes received, either from the violent motion of the earth alone, or that aſſiſted with the additional one of the volcanic exhalations ſet at liberty, ſeems to have acted with greater force on the lower and more compact ſtratum than on the upper cultivated cruſt: for I conſtantly obſerved, where theſe cultivated iſlands lay (for ſo they appeared to be on the barren bottom of the ravine) the under ſtratum of compact clay had been driven ſome hundred yards further, and lay in confuſed blocks, and, as I obſerved, many of thoſe blocks were of a cubical form. The under ſoil having had a greater impulſe, and leaving the upper in its flight, naturally accounts for the order in which the trees, vineyards, and vegetation, fell and remain at preſent in the bottom of the ravine. This curious fact, I thought, deſerved to be recorded, but is not eaſily deſcribed by words. When the drawings and plans of the Academy are publiſhed, this account (imperfect as it is) may, perhaps, have its utility: had my time permitted, I would certainly have taken a draughtſman with me into Calabria. In another part of the bottom of the ravine there is a mountain compoſed of the ſame clay ſoil, and which was probably a piece of the plain detached by an earthquake at ſome former period; it is about 250 feet high, and about 400 feet diameter at its baſis: this mountain, as is well atteſted, has travelled down the ravine near four miles, having been put in motion

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by the earthquake of the 5th of February. The abundance of rain which fell at that time, the great weight of the freſh detached pieces of the plain, which I ſaw heaped up at the back of it, the nature of the ſoil of which it is compoſed, and particularly its ſituation on a declivity, accounts well for this phenomenon; whereas the reports which came to Naples, of a mountain, in a perfect plain, having leaped four miles, had rather the appearance of a miracle. I found ſome ſingle timber trees alſo with a lump of their native ſoil at the roots, ſtanding upright in the bottom of the ravine, and which had been detached from the plain above mentioned. I obſerved alſo, that many confuſed heaps of the looſe ſoil detached by the earthquake from the plains on each ſide of the ravine, had actually run like a volcanic lava (having probably been aſſiſted by the heavy rain) and produced many effects greatly reſembling thoſe of lava during their courſe down a great part of the ravine. At Santa Criſtina, in the neighbourhood of Oppido, the like phenomena have been exhibited, and the great force of the earthquake of the 5th of February ſeems to have been exerted on theſe parts and at Caſal Nuova and Terra Nuova. The phenomena exhibited by the earthquakes in other parts of the plains of Calabria Ultra are of the ſame nature; but trifling in compariſon of thoſe I have been deſcribing. The barracks erected for the remaining inhabitants of the ancient city of Oppido, now in ruins, are on a healthy ſpot, at about the diſtance of a mile from the old town, where I found the baron of this country, the Prince of Cariati, uſefully employed in the aſſiſtance of his unfortunate ſubjects. He ſhewed me two girls, one of about ſixteen years of age, who had remained eleven days without food under the ruins of a houſe at Oppido: ſhe had a child of five or ſix months old in her arms,

which

which died the fourth day. The girl gave me a clear account of her ſufferings; having light through a ſmall opening, ſhe had kept an exact account of the number of days ſhe had been buried. She did not ſeem to be in bad health, drinks freely, but has yet a difficulty in ſwallowing any thing ſolid. The other girl was about eleven years of age; ſhe remained under the ruins ſix days only; but in ſo very confined and diſtreſsful a poſture, that one of her hands, preſſing againſt her cheek, had nearly worn a hole through it.

From Oppido I proceeded through the ſame beautiful country and ruined towns and villages to Seminara and Palmi. The houſes of the former were not quite in ſuch a ruined condition as thoſe of the latter, whoſe ſituation is lower and nearer the ſea. 1400 lives were loſt at Palmi, and all the dead bodies have not been removed and burnt, as in moſt other parts I viſited; for I ſaw myſelf two taken up whilſt I was there, and I ſhall ever remember a melancholy figure of a woman in mourning, ſitting upon the ruins of her houſe, her head reclined upon her hand and knee, and following with an anxious eager eye every ſtroke of the pick-axe of the labourers employed to clear away the rubbiſh, in hopes of recovering the corpſe of a favourite child. This town was a great market for oil, of which there were upwards of 4000 barrels in the town at the time of its deſtruction, ſo that the barrels and jars being broken, a river of oil ran into the ſea from it for many hours. The ſpilt oil mixed with the corn of the granaries, and the corrupted bodies, have had a ſenſible effect on the air. This I fear, as the heats increaſe, may prove fatal to the unfortunate remainder of the inhabitants of Palmi, who live in barracks near the ruined town. My guide told me, that he had been buried in the ruins of his houſe here by the firſt ſhock, and

that after the ſecond, which followed immediately, he found himſelf ſitting aſtride of a beam at leaſt fifteen feet high in the air. I heard of many ſuch extraordinary eſcapes in all parts of the plain, where the earthquake has exerted its greateſt force.

From Palmi I proceeded through the beautiful woody mountains of Bagnara and Solano; noble timber oak trees on high rocks, narrow vallies with torrents in their bottoms, the road dangerous both on account of robbers and precipices. My two guards, inſtead of leading the way, as they had hitherto done, now ſeparated and formed an advanced and a rear-guard. The narrow road was often interrupted by the fallen rocks and trees during the earthquakes, and obliged us to ſeek a new and ſtill more dangerous road; but the Calabreſe horſes are really as ſure-footed as goats. In the midſt of one of theſe paſſes we felt a very ſmart ſhock of an earthquake, accompanied by a loud exploſion, like that of ſpringing a mine: fortunately for us it did not, as I expected, detach any rocks or trees from the high mountains that hung over our heads. After having paſſed the woods of Bagnara, Sinopoli, and Solano, I went through rich corn-fields and lawns, beautifully bounded with woods and ſcattered trees, like our fineſt parks, and which continue varying for ſome miles till you come upon the top of an open plain on a hill, commanding the whole Faro of Meſſina, the coaſt of Sicily as far as Catania, with Mount Etna riſing proudly behind it, which altogether compoſed the fineſt view imaginable. From thence I deſcended a horrid rocky road to the Torre del Pezzolo, where there is a country-ſeat and a village belonging to the Princeſs of Bagnara. There I found, that an epidemical diſorder had already manifeſted itſelf, as it probably will in many other parts of this glorious but unhappy country,

in proportion as the heats increaſe, owing to the hardſhips ſuffered, and the air having been ſpoiled by new-formed lakes. Several fiſhermen aſſured me, that during the earthquake of the 5th of February at night, the ſand near the ſea was hot, and that they ſaw fire iſſue from the earth in many parts. This circumſtance has been often repeated to me in the plain; and my idea is, that the exhalations which iſſued during the violent commotions of the earth were full of electrical fire, juſt as the ſmoke of volcanoes is conſtantly obſerved to be during violent eruptions; for I ſaw no mark, in any part of my journey, of any volcanic matter having iſſued from the fiſſures of the earth; and I am convinced, that the whole damage has been done by exhalations and vapours only. The firſt ſhock felt at this place, as I was aſſured, was lateral, and then vorticoſe, and exceedingly violent; but what they call violent here, muſt have been nothing in compariſon of what was felt in the plain of Caſa Nuova, Poliſtene, Palmi, Terra Nuova, Oppido, &c. &c. where all agreed in aſſuring me, that the violence of the fatal ſhock of the 5th of February was inſtantaneous, without warning, and from the bottom upwards; and indeed in thoſe places, where the mortality has been ſo great, and where nothing is to be ſeen but a confuſed heap of ruins, without diſtinction of either ſtreets or houſes, the violence of that ſhock is ſufficiently confirmed. From this place to Reggio the road on each ſide is covered with villas and orange groves. I ſaw not one houſe levelled to the ground; but perceived that all had been damaged, and were abandoned; and that the inhabitants were univerſally retired to barracks in theſe beautiful groves of orange, mulberry, and fig-trees, of which there are many in the environs of Reggio. One that I viſited, and which is reckoned the richeſt in all this part of Magna Grecia, is about a mile and a half from the town of Reggio, and what

is remarkable, belongs to a gentleman whose Chriſtian name is Agamemnon. The beauty of the agrume (the general name of all kind of orange, lemon, cedrate, and bergamot-trees) is not to be deſcribed; the ſoil being ſandy, the expoſition warm, and command of water, a clear rivulet being introduced at pleaſure in little channels to the foot of each tree, is the reaſon of the wonderful luxuriancy of theſe trees. Don Agamemnon aſſured me, it was a bad year when he did not gather from his garden (which is of no great extent) 170,000 lemons, 200,000 oranges (which I found as excellent as thoſe of Malta), and bergamots enough to produce 200 quarts of the eſſence from their rinds. There is another ſingularity in theſe gardens, as I was aſſured, every fig-tree affords two crops of fruit annually; the firſt in June, the ſecond in Auguſt. But to return to my ſubject, from which my attention was frequently called away by the extraordinary and uncommon beauty and fertility of this rich province; I arrived about ſun-ſet at Reggio, which I found leſs damaged than I expected, though not a houſe in it is habitable or inhabited, and all the people live in barracks or tents; but after having been ſeveral days in the plain, where every building is levelled to the ground, a houſe with a roof, or a church with a ſteeple, was to me a new and refreſhing object. The inhabitants of the whole country, that has been ſo ſeverely afflicted with earthquakes, ſeem, however, to have ſo great a dread of going into a houſe, that when the earthquakes ſhall have ceaſed, I am perſuaded, the greateſt part of them will ſtill continue to live in barracks. The barracks here (except ſome few that are even elegant) are ill conſtructed, as are in general throughout the country all barracks of towns that have been ſo little damaged as to allow the inhabitants to flatter themſelves with a hope of being able to return to, and occupy,

their houſes again, when the preſent calamity is at an end. Reggio has been roughly handled by the earthquakes, but is by no means deſtroyed. The archbiſhop, a ſenſible, active, and humane prelate, has diſtinguiſhed himſelf from the beginning of the earthquakes to this day, having immediately diſpoſed of all the ſuperfluous ornaments of the churches, and of his own horſes and furniture, for the ſole relief of his diſtreſſed flock, with whom he chearfully bears an equal ſhare of every inconvenience and diſtreſs which ſuch a calamity has naturally occaſioned. Except in this inſtance, and very few others, indeed, I obſerved throughout my whole journey, a prevailing indolence, inactivity, and want of ſpirit, which is unfortunate, as ſuch a heavy and general calamity can only be repaired by a diſpoſition directly contrary to that which prevails; but as this government is indefatigable in its endeavours at remedying every preſent evil, and preventing ſuch as may naturally be expected, it is to be hoped that the generous and wiſe diſpoſitions lately made, will reſtore the energy that is wanting, and without which, one of the richeſt provinces in Europe is in danger of utter ruin. Silk and eſſence of bergamot, oranges and lemons, are the great articles of trade at Reggio. I am aſſured, that no leſs than 100,000 quarts of this eſſence is annually exported. The fruit, after the rind is taken off, is given to the cows and oxen; and the inhabitants of this town aſſure me, that the beef, at that ſeaſon, has a ſtrong and diſagreeable flavour of bergamot. The worthy archbiſhop gave me an account of the earthquakes here in 1770 and 1780, which obliged the inhabitants (in number 16,400) to encamp or remain in barracks ſeveral months, without however having done any conſiderable damage to the town. I was aſſured here (where they have had ſuch a long experience

rience of earthquakes) that all animals and birds are in a greater or leſs degree much more ſenſible of an approaching ſhock of an earthquake than any human being; but that geeſe, above all, ſeem to be the ſooneſt and moſt alarmed at the approach of a ſhock: if in the water, they quit it immediately, and there are no means of driving them into the water for ſome time after. The mortality here, by the late earthquake of the 5th of February, correſponds with the apparent degree of damage done to the town, and does not exceed 126. As it happened about noon, and came on gently, the people of Reggio had time to eſcape; whereas, as I have often remarked, the ſhock in the unhappy plain was as inſtantaneous as it was violent and deſtructive. Every building was levelled to the ground, and the mortality was general, and in proportion to the apparent deſtruction of the buildings. Reggio was deſtroyed by an earthquake before the Marſian war, and having been rebuilt by Julius Cæſar was called Reggio Julio. Part of the wall ſtill remains, and is called the Julian Tower; it is built of huge maſſes of ſtone without cement. Near St. Peruto, between Reggio and the Cape Spartivento, there are the remains of a foundery, his preſent Catholic Majeſty, when King of Naples, having worked ſilver mines in that neighbourhood; which were ſoon abandoned, the profit not having anſwered the expence. There are ſome towns in the neighbourhood of Reggio that ſtill retain the Greek language. About fifteen years ago, when I made the tour of Sicily, I landed at Spartivento in Calabria Ultra, and went to Bova, where I found that Greek was the only language in uſe in that diſtrict. On the 14th of May I left Reggio, and was obliged (the wind being contrary) to have my boats towed by oxen to the Punta del Pezzolo, oppoſite Meſſina, from whence the current wafted us with great expe-

dition indeed into the port of Meſſina. The port and the town, in its half ruined ſtate, by moon-light was ſtrikingly pictureſque. Certain it is, that the force of the earthquake (though very violent) was nothing at Meſſina and Reggio to what it was in the plain. I viſited the town of Meſſina the next morning, and found, that all the beautiful front of what is called the Palazzata, which extended in very lofty uniform buildings, in the ſhape of a creſcent, had been in ſome parts totally ruined, in others leſs; and that there were cracks in the earth of the quay, a part of which had ſunk above a foot below the level of the ſea. Theſe cracks were probably occaſioned by the horizontal motion of the earth in the ſame manner as the pieces of the plain were detached into the ravines at Oppido and Terra Nuova; for the ſea at the edge of the quay is ſo very deep, that the largeſt ſhips can lie alongſide; conſequently the earth, in its violent commotion, wanting ſupport on the ſide next the ſea, began to crack and ſeparate, and as where there is one crack there are generally others leſs conſiderable in parallel lines to the firſt, I ſuppoſe the great damage done to the houſes neareſt the quay has been owing to ſuch cracks under their foundations. Many houſes are ſtill ſtanding, and ſome little damaged, even in the lower part of Meſſina; but in the upper and more elevated ſituations, the earthquakes ſeem to have had ſcarcely any effect, as I particularly remarked. A ſtrong inſtance of the force of the earthquake having been many degrees leſs here than in the plain of Calabria is, that the convent of Santa Barbara, and that called the Noviziato de' Geſuiti, both on an elevated ſituation, have not a crack in them, and that the clock of the latter has not been deranged in the leaſt by the earthquakes that have afflicted this country for four months paſt, and which ſtill continue in ſome degree.

degree. Beſides, the mortality at Meſſina does not exceed 700 out of upwards of 30,000, the ſuppoſed population of this city at the time of the firſt earthquake, which circumſtance is concluſive. I found, that ſome houſes, nay a ſtreet or two, at Meſſina, were inhabited, and ſome ſhops open in them; but the generality of the inhabitants are in tents and barracks, which, having been placed in three or four different quarters, in fields and open ſpots near the town, but at a great diſtance one from the other, muſt be very inconvenient for a mercantile town; and unleſs great care is taken to keep the ſtreets of the barracks, and the barracks themſelves, clean, I fear that the unfortunate Meſſina will be doomed to ſuffer a freſh calamity from epidemical diſorders, during the heat of ſummer. Indeed, many parts of the plain of Calabria ſeem to be in the ſame alarming ſituation, particularly owing to the lakes, which are forming from the courſe of rivers having been ſtopped, ſome of which, as I ſaw myſelf, were already green, and tending to putrefaction. I could not help remarking here, that the nuns, who likewiſe live in barracks, were conſtantly walking about, under the tuition of their confeſſor, and ſeemed gay, and to enjoy the liberty the earthquake had afforded them, and I made the ſame obſervation with reſpect to ſchool-boys at Reggio; ſo that in my journal, which I wrote in haſte, and from whence I have as haſtily tranſcribed the imperfect account I ſend you, the remark ſtands thus: "*Earthquakes particularly pleaſing to nuns and ſchool-boys.*" Out of the cracks on the quay, it is ſaid, that during the earthquakes fire had been ſeen to iſſue (as many I ſpoke with atteſted); but there are no viſible ſigns of it, and I am perſuaded it was no more than, as in Calabria, a vapour charged with electrical fire, or a kind of inflammable air. A curious circumſtance happened here alſo,

to

to prove that animals can remain long alive without food. Two mules belonging to the Duke of Belviſo, remained under a heap of ruins, one of them twenty-two, and the other twenty-three days: they would not eat for ſome days, but drank water plentifully, and are now quite recovered. There are numberleſs inſtances of dogs remaining many days in the ſame ſituation; and a hen, belonging to the Britiſh vice-conſul at Meſſina, that had been cloſely ſhut up under the ruins of his houſe, was taken out the twenty-ſecond day, and is now recovered; it did not eat for ſome days, but drank freely; it was emaciated, and ſhewed little ſigns of life at firſt. From theſe inſtances, from thoſe related before, of the girls at Oppido, and the hogs at Soriano, and from ſeveral others of the ſame kind, that have been related to me, but which being leſs remarkable I omit, one may conclude, that long faſting is always attended with great thirſt, and total loſs of appetite. From every inquiry I found, that the great ſhock of the 5th of February was from the bottom upwards, and not like the ſubſequent ones, which in general have been horizontal and vorticoſe. A circumſtance worth remarking (and which was the ſame on the whole coaſt of the part of Calabria that had been moſt affected by the earthquakes) is, that a ſmall fiſh called Cicirelli, reſembling what we call in England white-bait, but of a greater ſize, and which uſually lye at the bottom of the ſea, buried in the ſand, have been ever ſince the commencement of the earthquakes, and continue ſtill to be, taken near the ſurface, and in ſuch abundance, as to be the common food of the pooreſt ſort of people; whereas, before the earthquakes, this fiſh was rare, and reckoned amongſt the greateſt delicacies. All fiſh, in general, have been taken in greater abundance, and with much greater facility, in thoſe parts ſince they have been afflicted by earthquakes

earthquakes than before. I conſtantly aſked every fiſherman I met with on the coaſt of Sicily and Calabria, if this circumſtance was true; and was as conſtantly anſwered in the affirmative; but with ſuch emphaſis, that it muſt have been very extraordinary. I ſuppoſe, that either the ſand at the bottom of the ſea may have been heated by the volcanic fire under it; or that the continual tremor of the earth has driven the fiſh out of their ſtrong holds, juſt as an angler, when he wants a bait, obliges the worms to come out of the turf on a river ſide, by trampling on it with his feet, which motion never fails in its effect, as I have experienced very often myſelf. I found the citadel here had not received any material damage; but was in the ſame ſtate as I had left it fifteen years ago. The Lazaret has ſome cracks in it, like thoſe on the quay, and from a like cauſe. The port has not received any damage from the earthquakes. The officer who commanded in the citadel, and who was there during the earthquake, aſſured me, that on the fatal 5th of February, and the three following days, the ſea, about a quarter of a mile from that fortreſs, roſe and boiled in a moſt extraordinary manner, and with a moſt horrid and alarming noiſe, the water in the other parts of the Faro being perfectly calm. This ſeems to point out exhalations or eruptions from cracks at the bottom of the ſea, which may very probably have happened during the violence of the earthquakes; all of which, I am convinced, have here a volcanic origin. On the 17th of May I left Meſſina, where I had been kindly and hoſpitably treated, and proceeded in my Speronara along the Sicilian coaſt to the point of the entrance of the Faro, where I went aſhore, and found a prieſt who had been there the night between the 5th and 6th of February, when the great wave paſſed over that point, carried off boats, and above

twenty-four unhappy people, tearing up trees, and leaving some hundred weight of fish it had brought with it on the dry land. He told me, he had been himself covered with the wave, and with difficulty saved his life. He at first said the water was hot; but as I was curious to come at the truth of this fact, which would have concluded much, I asked him if he was very sure of it? and being pressed, it came to be no more than the water having been as warm as it usually is in summer. He said, the wave rose to a great height, and came on with noise, and such rapidity that it was impossible to escape. The tower on the point was half destroyed, and a poor priest that was in it lost his life. From hence I crossed over to Scilla. Having met with my friend the Padre Minasi, a Dominican friar, a worthy man and an able naturalist, who is a native of Scilla, and is actually employed by the Academy of Naples to give a description of the phenomena that have attended the earthquake in these parts, with his assistance on the spot, I perfectly understood the nature of the formidable wave that was said to have been boiling hot, and had certainly proved fatal to the baron of the country, the Prince of Scilla, who was swept off the shore into the sea by this wave, with 2473 of his unfortunate subjects. The following is the fact. The Prince of Scilla having remarked, that during the first horrid shock (which happened about noon the 5th of February) part of a rock near Scilla had been detached into the sea, and fearing that the rock of Scilla, on which his castle and town is situated, might also be detached, thought it safer to prepare boats, and retire to a little port or beach surrounded by rocks at the foot of the rock. The second shock of the earthquake, after midnight, detached a whole mountain, (much higher than that of Scilla, and partly calcareous,

and partly cretaceous), ſituated between the Torre del Cavallo and the rock of Scilla. This having fallen with violence into the ſea (at that time perfectly calm) raiſed the fatal wave, which I have above deſcribed to have broken upon the neck of land, called the Punta del Faro, in the iſland of Sicily, with ſuch fury, which returning with great noiſe and celerity directly upon the beach, where the prince and the unfortunate inhabitants of Scilla had taken refuge, either daſhed them with their boats, and richeſt effects againſt the rocks, or whirled them into the ſea; thoſe who had eſcaped the firſt and greateſt wave were carried off by a ſecond and third, which were leſs conſiderable, and immediately followed the firſt. I ſpoke to ſeveral men, women, and children here, who had been cruelly maimed, and ſome of whom had been carried into the ſea by this unforeſeen accident. Here, ſaid one, my head was forced through the door of the cellar, which he ſhewed me was broken. There, ſaid another, was I drove into a barrel. Then a woman would ſhew me her child, all over deep wounds from the ſtones and timber, &c. that were mixed with the water, and daſhing about in this narrow port; but all aſſured me, they had not perceived the leaſt ſymptom of heat in the water, though I dare ſay, Sir, you will read many well atteſted acounts of this water having been hot; of many dead bodies thrown up which appeared to have been parboiled by it; and of many living perſons, who had evidently been ſcalded by this hot wave: ſo difficult is it to arrive at truth. Had I been ſatisfied with the firſt anſwer of the prieſt at the Punta del Faro, and ſet it down in my journal, who could have doubted but that this wave had been of hot water? Now that we are well acquainted with the cauſe of this fatal wave, we know it could not have been hot; but the teſtimony of ſo many unfortunate ſufferers

from it, is deciſive. A fact which I was told, and which was atteſted by many here, is very extraordinary indeed: a woman of Scilla, four months gone with child, was ſwept into the ſea by the wave, and was taken up alive, floating on her back at ſome diſtance, nine hours after. She did not even miſcarry, and is now perfectly well; and, had ſhe not been gone up into the country, they would have ſhewn her to me. They told me, ſhe had been uſed to ſwim, as do moſt of the women in this part of Calabria. Her anxiety and ſufferings, however, had arrived at ſo great a pitch, that juſt at the time that the boat, which took her up, appeared, ſhe was trying to force her head under water, to put a period to her miſerable exiſtence. The Padre Minaſi told me another curious circumſtance that happened in this neighbourhood, which to his knowledge was ſtrictly true. A girl of about eighteen years of age, was buried under the ruins of a houſe ſix days, having had her foot, at the ancle, almoſt cut off by the edge of a barrel that fell upon it; the duſt and mortar ſtopped the blood; ſhe never had the aſſiſtance of a ſurgeon; but the foot of itſelf dropped off, and the wound is perfectly healed without any other aſſiſtance but that of nature. If of ſuch extraordinary circumſtances, and of hair-breadth eſcapes, an account was to be taken in all the deſtroyed towns of Calabria Ultra and Sicily, they would, as I ſaid before, compoſe a large volume. I have only recorded a few of the moſt extraordinary, and ſuch as I had from the moſt undoubted authority. In my way back to Naples (where I arrived the 23d of May) along the coaſt of the two Calabrias and the Principato Citra, I only went on ſhore at Tropea, Paula, and in the Bay of Palinurus. I found Tropea (beautifully ſituated on a rock overhanging the ſea) but little damaged: however, all the inhabitants were in

barracks. At Paula the ſame. The fiſhermen here told me, they continued to take a great abundance of fiſh, as they had done ever ſince the commencement of the preſent calamity. At Tropea, the 15th of May, there was a ſevere ſhock of an earthquake, but of a very ſhort duration. There were five ſhocks during my ſtay in Calabria and Sicily; three of them rather alarming: and at Meſſina, in the night-time, I conſtantly felt a little tremor of the earth, which has been obſerved by many of the Meſſineſe. I am really aſhamed, Sir, of ſending ſuch an unconnected haſty extract of my journal; but when I reflect, that unleſs I ſend it off directly, the Royal Society will be broken up for the ſummer ſeaſon, and the ſubject will become ſtale before its next meeting; of two evils I prefer to chuſe the leaſt. Such rough draughts however (though ever ſo imperfect and incorrect) have, as in paintings, the merit of a firſt ſketch, and a kind of ſpirit that is often loſt when the picture is correctly finiſhed. If you conſider the fatigue and hurry of the journey I have juſt been taking; and that in the midſt of the preparations for my other journey to England, which I propoſe to begin to-morrow, I have been writing this account, I ſhall hope then to be entitled to your indulgence for all its imperfections *. But before I take my leave, I will juſt ſum up the reſult of my obſervations in Calabria and Sicily, and give you my reaſons for believing that the preſent earthquakes are occaſioned by the operation of a volcano, the ſeat of which ſeems to lye deep, either under the bottom of the ſea, between the iſland of Stromboli and the coaſt of Calabria, or under the parts of the plain towards Oppido and

* Quæramus ergo quid ſit quod terram ab infimo moveat, quid, &c. . . . . Hæc ex quibus cauſis accidant digna res eſt excuti. See the whole paſſage very applicable here. SENECA, Nat. Queſt. lib. VI. cap. 4.

Terra

Terra Nuova. If on a map of Italy, and with your compaſs on the ſcale of Italian miles, you were to meaſure off 22, and then fixing your central point in the city of Oppido (which appeared to me to be the ſpot on which the earthquake had exerted its greateſt force) form a circle (the radii of which will be, as I juſt ſaid, 22 miles) you will then include all the towns, villages, that have been utterly ruined, and the ſpots where the greateſt mortality has happened, and where there have been the moſt viſible alterations on the face of the earth. Then extend your compaſs on the ſame ſcale to 72 miles, preſerving the ſame center, and form another circle, you will include the whole of the country that has any mark of having been affected by the earthquake. I plainly obſerved a gradation in the damage done to the buildings, as alſo in the degree of mortality, in proportion as the countries were more or leſs diſtant from this ſuppoſed center of the evil. One circumſtance I particularly remarked, if two towns were ſituated at an equal diſtance from this center, the one on a hill, the other on the plain, or in a bottom, the latter had always ſuffered greatly more by the ſhocks of the earthquakes than the former; a ſufficient proof to me of the cauſe coming from beneath, as this muſt naturally have been productive of ſuch an effect. And I have reaſon to believe, that the bottom of the ſea, being ſtill nearer the volcanic cauſe, would be found (could it be ſeen) to have ſuffered even more than the plain itſelf; but (as you will find in moſt of the accounts of the earthquake that are in the preſs, and which are numerous) the philoſophers, who do not eaſily abandon their ancient ſyſtems, make the preſent earthquakes to proceed from the high mountains of the Apennines that divide Calabria Ultra, ſuch as the Monte Dejo, Monte Caulone, and Aſpramonte; I would aſk them

this

this ſimple queſtion, did the Æolian or Lipari iſlands (all which roſe undoubtedly from the bottom of the ſea by volcanic exploſions at different, and perhaps very diſtant, periods) owe their birth to the Apenines in Calabria, or to veins of minerals in the bowels of the earth, and under the bottom of the ſea? Stromboli an active volcano, and probably the youngeſt of thoſe iſlands, is not above 50 miles from the parts of Calabria that have been moſt affected by the late earthquakes. The vertical ſhocks, or, in other words, thoſe whoſe impulſe was from the bottom upwards, have been the moſt deſtructive to the unhappy towns in the plain; did they proceed from Monte Dejo, Monte Caulone, or Aſpramonte? In ſhort, the idea I have of the preſent local earthquakes is, that they have been cauſed by the ſame kind of matter that gave birth to the Æolian or Lipari iſlands; that, perhaps, an opening may have been made at the bottom of the ſea, and moſt probably between Stromboli and Calabria Ultra (for from that quarter all agree, that the ſubterraneous noiſes ſeem to have proceeded); and that the foundation of a new iſland or volcano may have been laid, though it may be ages, which to nature are but moments, before it is completed, and appears above the ſurface of the ſea. Nature is ever active; but her actions are, in general, carried on ſo very ſlowly, as ſcarcely to be perceived by mortal eye, or recorded in the very ſhort ſpace of what we call hiſtory, let it be ever ſo ancient. Perhaps too, the whole deſtruction I have been deſcribing may have proceeded ſimply from the exhalations of confined vapours, generated by the fermentation of ſuch minerals as produce volcanoes, which have eſcaped where they met with the leaſt reſiſtance, and muſt naturally in a greater degree have affected the plain than the high and more ſolid grounds around it. When the account of the

Royal

Royal Academy of Naples is publiſhed, with maps, plans, and drawings, of the curious ſpots I have deſcribed, this rude and imperfect account will, I flatter myſelf, be of uſe: without the help of plans and drawings you well know, Sir, the great difficulty there is in making one's ſelf intelligible on ſuch a ſubject. The incloſed letter, which I received whilſt I was in Calabria Ultra, from the Marquis Ippolito, a gentleman of Catanzaro, and an able naturaliſt, will give you the particulars of the phenomena that have been produced by the late earthquakes in Calabria Citra, my time having permitted me to viſit only a part of that province. I once more then crave your kind indulgence, and that of the members of our reſpectable Society, if you ſhould think proper to communicate this haſty paper to them.

I have the honour to be, &c.

XII. *Account of the Earthquake which happened in* Calabria, March 28, 1783. *In a Letter from Count* Franceſco Ippolito *to Sir* William Hamilton, *Knight of the Bath, F. R. S.; preſented by Sir* William Hamilton.

Read July 10, 1783.

EXCELLENZA,

QUESTA parte del noſtro regno di Napoli, occupata un tempo da Bruzi, e da molte Greche colonie, che ora Calabria ſi appella, è ſtata ſempre un funeſto teatro del terribile fenomeno, che attualmente ſi oſſerva e ſi ſoffre. E pur recente la memoria de' tremuoti avvenuti nel 1638 e nel 1659 per i quali le due provincie della Calabria rimaſero quaſi interamente deſtrutte. Piu freſca è l'idea che abbiamo del tremuoto accaduto nel 1743 in 44 che affliſſe la noſtra nazione per lungo ſpazio di tempo, mà ſenza ruina di città, ne perdita di cittadini. In Reggio, e ne Paeſi all' intorno, i tremuoti quaſi ogni anno ſi fanno ſentire, che ſe riandar vogliamo le antiche memorie, noi trovaremo nè ſecoli remoti, e della piu oſcura antichità, che tutta l'Italia, mà con iſpezialità il noſtro regno, e ſopra tutto le noſtre provincie, anno ſofferte varie cataſtrofi avvenute per li fuochi volcanici, e per lo ſcoppio delle ſotterranee accenſione. Il culto religioſo de Bruzi, per quanto dalle Storie rilevaſi, veſtito di triſtezza, e di lutto, dimoſtra le funeſte impreſſioni, che nell' animo di quei popoli, noſtri antenati, faceva

la ſpeſſa, ed orribile convulſione della terra. Ne poteva, ne può altrimente avvenire in queſte noſtre regioni: eſſendo eſſe interſecate dalla Catena degli Appennini, nel ſeno de quali altro non trovanſi, che Zolfi, Ferro, Carboni foſſili, Olio petrolo, ed altre materie bituminoſe, e combuſtibili. Fra i molti minerali, di cui abbondano, di facile debbono farſi delle fermentazioni, ed accenzioni ſotterranee. Buon per noi, che abbiamo nelle noſtre vicinanze varii volcani, i quali ſervir poſſono di camini, per dar libero eſito, e sfogo al fuoco che ſotto à noſtri piedi ſi accende!

Frà tanti famoſi Tremuoti, che queſte noſtre regioni hanno ſofferti, occupar non dée l'ultimo luogo quello, a cui ora ſoggiacia, o che ſi riguardi la forza delle concuſſioni, e a di loro durata, o che ſi riguardino i cambiamenti prodotti nella ſoperfizie della terra e la ruina di tante città e ville, colla perdita di circa quarantamila abbitanti.

Io dacché ſi ſentì la prima orribile ſcoſſa de 5 Febraio, cominciai a notare in un diario non ſolo le convulſioni, che giornalmente ſi ſono dalla terra patite, mà eziandio le meteore tutte, che da quell' epoca in qua ſi ſono giornalmente oſſervate nell' atmosfera. L'anguſtia del tempo non mi permette di traſcriverlo, e mandarlo a voſtra eccellenza, mà reſtringendo in poche parole quanto nel medeſimo ſi contiene, poſſo aſſicurarla, che dal cennato di del 5 Febbraio inſino a queſt' ora le ſcoſſe ſono ſtate ſempre ſpeſſiſſime, e quaſi ogni giorno, or ondolando la terra, or ſocotendoſi, or movendoſi vorticoſamente in maniera, che ci parea di eſſere allora ſopra un naviglio agitato, e commoſſo da flutti tempeſtoſi. In queſta ſerie continuata di tremuoti non debbo laſciare di far oſſervare a voſtra eccellenza, che le più notabili, e grandi accenſioni ſono ſeguite a di 5 Febbraio circa le ore 19½ d'Italia; a di 7 dello ſteſſo meſe verſo le ore 20½,

a di 28 del ſud. Febbraio circa le ore 8½ della notte ſequente, e finalmente a di 28 di Marzo verſo l'ora 1½ della ſera. Tutte le ſudette quattro accenſioni, per quanto puoſſi da fenomeni, e dagli effetti prodotti conjetturare, procedendo ſempre dalla catena de Monti, che da Reggio a noi vengono, hanno prodotto quattro diverſe eſploſioni in quattro luoghi diverſi della Calabria. Le tre prime furono in quella parte della provincia, in cui voſtra Eccellenza di preſente ſi trova, e per cui dovrà ella paſſare andando in Meſſina. Queſte eſploſioni diverſi funeſti effetti produſſero. Città, e ville rovinate, monti ſcoppiati, grandi fenditure di terra, nuovi gorghi di acque, antichi rivoli ſprofondati e diſperſi, fiumi trattenuti, terreni abbaſſati, piccioli monticelli nuovamente formati, piante ſbarbicate e lungi traſportate dal primiero loro ſito, terre vedute muoverſi rotolando per lungo tratto, animali ed uomini dalla terra ingojati. Mà io mi aſtengo di farne a voſtra excellenza un minuto dettaglio. Ella colla propria iſpezione, e colle relazioni de teſtimoni oculari di tali fenomeni, che coſtà potrà facilmente avere, ſe ne formerà una ſtoria veridica; non poſſo però nè debbo laſciar di dirle, che fra tutt' i fenomeni in coteſte parte accaduti, il più notabile è quello che ne' lidi di Scilla e di Bagnara avvenne. Quella parte di mare, che trabboccò ſtraordinariamente in quelle maremme, ed ingojò più migliaja di uomini, che quivi ſi erano rifuggiati, ſi ſentì talmente calda, che ſcottò quei pochi, che dall improviſo inondamento ſalvaronſi, ſiccome anche per bocca dell' eccellentiſſimo Sig. Vicario Generale ho ſaputo.

Io dunque mi tratterrò ſolo a narrarvi in brieve gli effetti dell' ultima eſploſione de 28 d. Marzo, la quale, ſenza altro dovette avvenire da un' accenſione fattaſi nelle viſcere della terra in queſti noſtri contorni, e preciſamente ne' Monti, che vengono a traverſare il collo della noſtra Peniſola formata dai due fiumi, Lameto, il

quale imbocca nel golfo di St. Eufemia, e Corace, che ſcorre nel mare Ionio, e proprio nel ſeno di Squillace. Tanto ne dimoſtrano i fenomeni da queſta ultima concuſſione prodotti, de quali ho l'onore di fare un dettaglio a voſtra eccellenza.

Queſta ſcoſſa, come tutte le altre, dimoſtrò farſi per la direzione di Ponente-Lebeccio venendo a noi. Incominciò ſulle prime ad ondolare la terra, quindi ſi concuſſe, finalmente ſi moſſe vorticoſamente in guiſa, che molti ſtando all' impiedi, o caddero, o mal poterono reggerſi ſulle piante. Durò la terribile convulſione circa minuti dieci ſecondi, alla quale ſucceſſero delle altre men forti, di minor durata, e di ſola ondolazione, coſichè per tutta la notte, e per la metà del giorno ſeguente, prima per ogni 5 minuti, e poi per ogni quadrante di ora la terra ſi moſſe.

Un terribile ſotterraneo muggito precedette alquanto, ed accompagnò la ſopradetta concuſſione, la quale terminò finalmente con un tuono piu intenſo ſimile a quello che ſi farebbe da una mina, che ſcoppia. Simili tuoni accompagnarono coſtantemente non ſolo le ſcoſſe avvenute nella notte, e giorno ſeguente, mà quelle eziandio, che ſi ſono ſino a queſt' ora ſentite; che anzi ſi ſono uditi alle volte de mugiti ſenza concuſſione alcuna di terra, e prima di 28 Marzo delle noſtre vicine Montagne s'udivano ſtrepiti e fragori, come tanti ſpari di Bombarde.

L'aere era coverto di nuvole, ed agitato da gagliardi venti occidentali, i quali poco prima, che ſeguiſſe l'orrenda ſcoſſa, eranſi tutt'à un colpo calmati: ma immediatemente dopo di queſta anche tutt' à un colpo riſorſero, e poco dopo quietaronſi. Vi furono non dimeno nell' atmoſfera in tutta quella notte delle ſpeſſe, e ſubitanee mutazioni, ora nuvoloſo il Cielo moſtrandoſi, ora ſereno, ora ſpirando un vento, ora un altro, ſempre però dalla plaga ripoſta tra ſud e Oueſt.

In

In quella notte medesima nelle vicinanze di questa città verso la marina, in cui si estese l'esplosione, nell' atto del tremuoto si veddero delle fiammelle uscir dalla terra, in maniera, che vari contadini sorpresi da timore, si diedero alla fuga: e queste fiamme si videro precisamente sortire da un luogo, da cui giorni avanti un certo straordinario calore tramandavasi.

Dopo la gran concussione comparve nell' aria verso l'oriente una fiamma bislunga, albicante, simile al fuoco elettrico, che si sostenne per lo spazio di due ore in circa.

Al terribile scoppio vari Paesi, e città rimasero abbattute, e specialmente quelli, che sono situati nelle vicinanze, e nel collo stesso della nostra Penisola, procedendo da Tiriolo fino al fiume Angitola, e che niun danno aveano da precedenti tremuoti sofferto. Curinga, Majda, Cortale, Girifalco, Borgia, St. Floro, Settingiano, Marcellinara, Tiriolo, ed altri Paesi di minor conto furono pressochè interamente distrutti mà con perdita di poca gente, se non che in Majda, Cortale, e Borgia più centinaja rimasero vittime delle ruine.

Quelli stessi effetti che i primi tremuoti cagionato aveano in quei luoghi ove trovasi vostra eccellenza furono anche prodotto dall' ultimo tremuoto in queste nostre Contrade. Furono molte Colline ove aperte, ove spianate. Molte aperture incontransi fatte nella terra per tutta la superfizie riposta tra le due valli de fiumi Corace, e Lameto procedendo verso il fiume Angitola. Da molte di queste aperture sgorgò per più ore acqua copiosa tramandata, o dalle sotterranee concamerazioni, o dall' istessi fiumi, vicino a quali venne a svenarsi la terra. Da una di esse fatta nel territorio di Borgia, distante circa un miglio dal mare, uscì copiosamente dell' acqua salza per più giorni, la quale sgorgando imitava i vari moti dell' onda stessa marina. Dalle aperture fattesi nelle pianure di Majda sgorgò dell' acqua calda;

mà

mà dir non ſo, ſe foſſe ſtata termale, o pur riſcaldata dalla ſteſſa ſotterranea accenzione.

E ancor d'avvertirſi, che da quell iſteſſe feſſure, dalle quali apparve ſgorgar dell' acqua, ſi tramandò dell' arena tenuiſſima, ove biggia, ove gialliccia, ove biancaſtra, la quale per la ſua eſtrema ſottiglieza ſembra quaſi un Sabbione. Di ſi fatte arene ho io avuta la ſola biggia, nelle quale vedeſi chiaramente framiſchiata parte di ferro.

Si è in oltre oſſervato, che nelle parti arenoſe, ove ſi è fatta l'eſploſione, tratto tratto s'incontrano delle aperture in forma di cono inverſo, dalle quali è anche uſcita dell' acqua; loché par che dimoſtri eſſere quindi ſcappato un fiocco di fuoco elettrico. Feſſure di tal forma s'incontrano ſpecialmente lungo le rive del fiume Lameto dalla ſua imboccatura in quá per più miglia.

Trá fenomeni, che precedettero, e ſeguirono il tremuoto de 28 Marzo ſono degni di oſſervazione i due ſeguenti. Nel giorno ſteſſo in Majda l'acqua ſorgiva di un pozzo, che prima beveaſi, ſi trovò infetta di un diſguſtevole ſapore ſulfureo, che anche proibiva il fiutarla. In Catanzaro all' incontro dopo il tremuoto ſudetto l'acqua di un pozzo, che prima non potea uſarſi, perche di un ſapor calcinoſo, ſi è poſcia reſa puriſſima, ſicche beveſi felicemente. In Majda ſteſſa nel tremuoto de 28 molte fontane ſi diſſeccarono, come è avvenuto ben anche in altre luoghi: mà molte altre ne ſono ſcaturite in varie parti, ove non erano altra volta comparſe; che anzi ſono comparſe delle nuove ſorgenti minerali, di cui non vi era veſtiggio, come è avvenuto in Cropani Paeſe del Marcheſato. Mà ordinariamente le fontane ſi ſono reſe più gonfie, e più copioſe, gittando acqua in un maggior volume del ſolito.

Si

Si ſono anche oſſervate le acque delle fontane reſe torbide, e di colore, ove bianchiccio, ed ove giallaſtro, ſecondo la natura de' terreni per dove ſcorrevano.

Varie elevazioni di terra ſeguite ſono per lo tremuoto medeſimo; la più notabile è quella che avvenne nel letto del fiume di Borgia, ove alzata ſi vede una nuova collinetta, alta circa tredici palmi, larga venti nella ſua baſe, e lunga circa ducento palmi. Finalmente nelle vicinanze del fiume Lameto, e preciſamente nell' diſtretto della terra detta di Amato, tutto ſbarbicato dal tremuoto, vedeſi un oliveto, e vedeſi ancora, che la ſuperfizie di tal fondo ne fù rivoltata vorticoſamente, come in coteſte contrade in varii luoghi avenne nè primi tremuoti.

Queſti ſono i più notabili fenonemi avvenuti in queſta noſtra reggione per lo fatale tremuoto de 28 Marzo, e che giunte ſono fin ora a mia notizia. Mi credo però nell' obligo di ſoggiungere a voſtra eccellenza, che queſta funeſta cataſtrofe dell' afflitta noſtra provincia, fù preceduta da grandi, ed inſoliti giacci avvenuti nell' inverno del 1782; da ſtraordinaria ſiccità, e da inſoffribili calori nella primavera ed eſtate del medeſimo anno; da grandi, copioſe, e continue pioggie cadute nell' autunno, e continuate per tutto Gennaio dell' anno corrente. Fra queſte dirotte pioggie, non iſcoppiò quaſi mai tuono ne fulmine; ed in queſta città, in cui ſogliono ſempre ſoffiar con gagliardia i venti, per tutto queſto tratto di tempo, o di raro, o leggiermente ſi fecero ſentire; mà poi nel principiar del tremuoto, diſſerraronſi con maſſimo impeto, e furore, accompagnati ora da pioggia, ora da gragnuola. Molto tempo prima che ſi ſcoteſſe la terra, videſi il mare gonfio ed elevato, ſenza che vi fuſſe vento, onde veniſſero le ſue onde agitate, in guiſa che i peſcatori ſteſſi non ardivano di entrarvi. I noſtre volcani per

quanto

quanto affeverantemente mi ſi è riferito, per lungo ſpazio prima non aveano affatto eruttato. Mà l'Etna eruttò nè primi tremuoti, e Stromboli fece vedere il ſuo fuoco negli ultimi. Or voglia il Cielo, che la terra rimettaſi al fine nella ſua perduta fermezza, ed abbia a tornare a noi il perduto equilibrio tanto nell' ordine fiſico, che morale. In tanto con profondo riſpetto mi raſſegno.

Di voſtra eccellenza, &c.

XIII. *Account of the* Black Canker Caterpillar, *which deſtroys the Turnips in* Norfolk. *By* William Marſhall, *Eſq. in a Letter to* Charles Morton, *M. D. F. R. S.*

Read February 8, 1783.

SIR,

Gunton, near Aylſham, Norfolk, Auguſt 22, 1782.

A FEW months after you did me the honour of preſenting my minutes of agriculture to the Britiſh Muſeum, I came down into Norfolk, as agent to Sir HARBORD HARBORD.

To a perſon intelligent in matters of agriculture it would be ſuperfluous to ſay, that Norfolk is celebrated for good huſbandmen; or that the turnip crop is the baſis of the Norfolk huſbandry. If a Norfolk farmer loſes his crop of turnips, his farm is injured for ſeveral ſucceeding years; for it is not only the loſs of the immediate profit, which would otherwiſe have ariſen to him from his bullocks, but his land is deprived of the conſequent manure and trampling (eſteemed highly beneficial to the light lands of this county) on which his future crops of corn are eſſentially dependant.

Among the numerous enemies to which turnips are liable, none have proved more fatal here than the Black Canker (a ſpecies of Caterpillar) which in ſome years have been ſo numerous as to cut off the farmer's hopes in a few days. In other years, however, the damage has been little, and in others nothing. About twenty years ago the whole country

was nearly ſtripped; and this year it has been ſubjected to a ſimilar fate. Many thouſands of acres, upon which a fairer proſpect for a crop of turnips has not been ſeen for many years, have been plowed up; and as, from the ſeaſon being now far ſpent, little profit can be expected from a ſecond ſowing; the loſs to the farmers, individually, will be very conſiderable, and to the county immenſe.

It was obſerved in the canker-year above mentioned, that, prior to the appearance of the caterpillars, great numbers of yellow flies were ſeen buſy among the turnip plants; and it was then ſuſpected, that the canker was the caterpillar ſtate of the yellow fly; and ſince that time it has been remarked, that cankers have regularly followed the appearance of theſe flies. From their more frequently appearing on the ſea-coaſt, and from the vaſt quantities which have, I believe, at different times, been obſerved on the beach waſhed up by the tide, it has been a received opinion among the farmers, that they are not natives of this country, but come acroſs the ocean, and obſervations this year greatly corroborate the idea. Fiſhermen upon the eaſtern coaſt declare, that they actually ſaw them arrive in cloud-like flights; and from the teſtimony of many, it ſeems to be an indiſputable fact, that they firſt made their appearance on the eaſtern coaſt; and, moreover, that on their firſt being obſerved, they lay upon and near the cliffs ſo thick and ſo languid, that they might have been collected into heaps, lying, it is ſaid, in ſome places two inches thick. From thence they proceeded into the country, and even at the diſtance of three or four miles from the coaſt they were ſeen in multitudes reſembling ſwarms of bees. About ten days after the appearance of the flies, the young caterpillars were firſt obſerved on the under ſides of the leaves of the turnips, and in

ſeven

ſeven or eight days more, the entire plants, except the ſtronger fibres, were eaten up. A border under the hedge was regularly ſpared until the body of the incloſure was finiſhed; but this done, the border was ſoon ſtripped, and the gateway, and even the roads have been ſeen covered with caterpillars travelling in queſt of a freſh ſupply of turnips; for the graſſes, and indeed every plant, except the turnip and the charlock (ſinapis arvenſis) they entirely neglect, and even die at their roots, without attempting to feed upon them. This deſtruction has not been confined within a few miles of the eaſtern coaſt, but has reached, more or leſs, into the very center of the county. The miſchief, however, in the weſtern parts of Norfolk, and even on the north coaſt, has been leſs general; but I am afraid it may be ſaid, with a great deal of truth, that one half of the turnips in the county have been cut off by this voracious animal

A circumſtance ſo diſcouraging to induſtry, and injurious to the public at large, will, I flatter myſelf, Sir, be thought a ſufficient apology for my troubling you with a relation of it, and for my taking the liberty of ſending you a male and a female fly, alſo one of the animals in its caterpillar, and one which is in its chryſalis ſtate, for your inſpection, hoping that the public may become acquainted with the means of preventing in future ſo great a calamity.

Leſt the flies may become diſfigured in travelling, it may be prudent to ſay, that their wings are four; that their antennæ are clubbed, and about one-third of the length of their body, each being compoſed of nine joints, namely, two next the head, above which two there is a joint ſomewhat longer than the reſt, and above this ſix more joints, ſimilar to the two below; that near the point of the tail of the female there is a

black ſpeck, outwardly fringed with hair; but which, opening longitudinally, appears to be the end of a caſe, containing a delicate point or ſting (about one-twentieth of an inch in length) which on a curſory view appears to be a ſimple lanceolated inſtrument, with a ſtrong line paſſing down the middle, and ſerrated at its edges; but, on a cloſer inſpection, and by agitating it ſtrongly with the point of a needle, it ſeparates into three one-edged inſtruments, hanger-like as to their general form, with a ſpiral line or wrinkle winding from the point to the baſe, making ten or twelve revolutions, which line, paſſing over their edges, gives them ſome appearance of being ſerrated.

By the help of theſe inſtruments, I apprehend, the female depoſits her eggs in the edge of the turnip-leaf (or ſometimes, perhaps, in the nerves or ribs on the under ſurface of the leaf); thus far I can ſay, and I think with a conſiderable degree of certainty, that having put ſome freſh turnip leaves into a glaſs containing ſeveral of the male and female flies, I perceived (by the means of a ſimple magnifier) that one of the females, after examining attentively the edge of the leaf, and finding a part which appeared to me to have been bitten, unſheathed her inſtruments, inſinuated them into the edge of the leaf, and having forced them aſunder ſo as to open a pipe or channel between them, placed her pubes (the ſituation of which from repeated and almoſt inceſſant copulations I had been able to aſcertain preciſely, and to the lower part of which theſe inſtruments ſeem to be fixed) to the orifice, and having remained a few ſeconds in that poſture, deliberately drew out the inſtruments (which the tranſparency of the leaf held againſt a ſtrong light afforded me an opportunity of ſeeing very plainly) and proceeded to ſearch for another convenient place for her purpoſe.

The

The caterpillar has twenty feet (ſix of its legs being of conſiderable length, the other fourteen very ſhort) and in its firſt ſtage is of a jetty black, ſmooth as to a privation of hair, but covered with innumerable wrinkles. Having acquired its full ſize, it fixes its hinder parts firmly to the leaf of a turnip, or any other ſubſtance, and breaking its outer coat or ſlough near the head, crawls out, leaving the ſkin fixed to the leaf, &c. The under coat, which it now appears in, is of a blueiſh or lead colour, and the caterpillar is evidently diminiſhed in its ſize. In every reſpect it is the ſame animal as before, and continues to feed on the turnips for ſome days longer: it then entirely leaves off eating, and becomes covered with a dewy moiſture, which ſeems to exſude from it in great abundance, and appearing to be of a glutinous nature, retains any looſe or pliant ſubſtance which happens to come in contact with it, and by this means alone ſeems to form its chryſalis coat. One I find laid up in the fold of a withered turnip leaf (that which I have the honour of incloſing you) was, among ſix others, formed by putting common garden mould to them while they were in the exſudatory ſtate above deſcribed.

From the generic characters of the fly I conclude it to be a Tenthredo of HILL; but whether that voluminous author be ſufficiently accurate; or whether, from being an almoſt entire ſtranger to natural hiſtory, I may, or may not, ſufficiently underſtand my book, I muſt beg leave to ſubmit to your ſuperior knowledge of the ſubject.

I am endeavouring to extend my obſervations on theſe inſects, and am making ſome experiments concerning them, the reſult of which I ſhould be extremely happy in being permitted to communicate to you; and it may be proper to add here, that

I ſhould not have taken the liberty of troubling you prematurely with this letter, had I not luckily met with an opportunity of procuring ſome live flies (which are now become very ſcarce); and I flatter myſelf they will come to your hands in a perfect ſtate.

I am, with the greateſt reſpect, &c.

XIV. *A Letter from Mr.* Edward Nairne, *F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S. containing an Account of Wire being ſhortened by Lightning.*

Read February 3, 1783.

SIR,

IN the Philoſophical Tranſactions for the year 1780, vol. LXX. are printed ſome experiments of mine, ſhewing the method of ſhortening of wire by the effect of electricity. I have ſince met with a ſimilar circumſtance produced by lightning; and, if the following account ſhould meet with your approbation, ſhould be happy to have it communicated to the Society.

On the 18th of June, 1782, Mr. PARKER's houſe at Stoke Newington was ſtruck by lightning, between two and three o'clock in the afternoon. The lightning paſſed down the leaden pipe without ſide the houſe, which pipe did not reach to the ground by about ten feet. Here the lightning ſtruck from one of the nails which faſtened this leaden pipe to the wall to the end of a crank iron that was drove in the wall oppoſite it, withinſide the room, and to which was faſtened the wire of a night-bolt, rather thicker than uſual. This wire was ſo very looſe before the accident happened, that the bolt could not be raiſed by the handle at the bed-ſide, ſo that they were obliged every night to take hold of the bolt itſelf to lift it up to faſten the door; but on the night after the accident had happened, they, on going to bed, went to raiſe the bolt up as uſual, to ſecure their chamber-door, when, to their great

great ſurprize, they found the bolt drawn up; and on trying to pull it down, they could not with all their ſtrength. It being a particular acquaintance of mine, they ſent for me. I went the next day, and not only found the bolt drawn up, but the wire, which they told me before was very looſe, and much bent, was drawn very ſtraight, and ſo tight, that when ſtruck it produced a muſical tone. The wire was judged to be ſhortened ſeveral inches; for, had the wire before the accident been ſtraight, it muſt have ſhortened it above two inches to have drawn the bolt up.

The whole length of the wire from the bed-ſide to the bolt was about thirty feet; but the part of the wire on which the lightning paſſed was about fifteen feet.

Near the crank iron that was directly over the bolt were two wires, which paſſed through the wainſcot to a ſingle one belonging to an alarm. The lightning paſſed theſe two wires, without damaging them; but the ſingle one was partly diſperſed into ſmoke, blackening all the wainſcot near it; alſo a great deal was melted into globules, which globules we found by a magnet.

This was the firſt inſtance (and, I muſt own, it agreeably ſurprized me) that I had ever met with of wire being contracted or ſhortened by the effect of lightning, though I have now not the leaſt doubt, but that it is always the caſe; and that is the reaſon that we find them moſtly broke where the ghtning has paſſed, if it does not melt them. I have often ſhortened wire by electricity, an account of which I gave to the Royal Society as before mentioned.

I have brought a piece of wire belonging to the night-bolt, and alſo ſome of the globules, for the inſpection of the Society.

I have the honour to be, &c.

POST-

POSTSCRIPT.

Being deſirous of knowing whether the lightning had any ways altered the property of the iron by melting it into globules, I applied to the hon. Mr. CAVENDISH, who very obligingly tried them with different acids, and found that they ſcarcely ſhewed any ſigns of effervescence even when heated over the fire. He next tried ſome iron filings, which he put to ſome of the ſame acid; theſe not only cauſed an effervescence, but were intirely diſſolved.

He alſo tried the pieces of ſteel ſtruck off by ſtriking a light, which being ſeparated by a magnet from the pieces of flint effervesced with the ſame acids, and diſſolved almoſt intirely, only half a grain being left out of eighteen, and theſe conſiſted principally of thoſe parts that were melted in globules.

XV. *An Account of Ambergriſe, by Dr.* Schwediawer; *preſented by Sir* Joſeph Banks, *P. R. S.*

Read February 13, 1783.

AMBERGRISE, or properly ſpeaking *Grey Amber*, is a ſolid, opaque, inflammable ſubſtance, of a white grey, ſometimes of a blackiſh colour, which melted or inflamed yields a peculiar ſmell, agreeable to moſt perſons, but diſagreeable to others.

As it occurs in the ſhops, it varies in its conſiſtence, according as it has been expoſed to a warmer or colder air. It is a hard brittle ſubſtance, yet not ſo hard as to admit a poliſh; nor has it, like ſuccinum, a poliſhed appearance or tranſparency. On ſcraping it with a knife into powder, part of it adheres to the cold ſteel like wax; ſo it does alſo to the teeth, if maſticated; it yields alſo the impreſſion of the nail; it has no peculiar but rather an earthy taſte when chewed.

It has in its natural ſtate a peculiar ſtrong ſmell. The older it grows the more it ſeems to become agreeable. This ſmell is rendered more ſenſible by rubbing it with the fingers, or by burning or melting it.

It

It melts in a moderate degree of heat into a blackiſh thick oil, and then ſmoaks, ſkums, and flies by degrees entirely off, without leaving any coal behind; ſo it does likewiſe when put upon any heated metal, leaving only a black ſpot upon it: when the metal is red-hot, it melts and inflames inſtantaneouſly, ſmoaks ſtrongly, and flies ſpeedily off, without leaving the leaſt mark behind. When brought near a burning candle it catches fire immediately, and burns with a clear bright flame till it is conſumed. A red-hot needle eaſily penetrates through its ſubſtance, a blackiſh oil then exſudes, but no part of it ſeems to adhere to the needle; the needle, however, feels afterwards as if it had been put into wax.

It is ſo light, that it ſwims not only upon the ſea, but alſo on the ſurface of freſh water.

Its colour is white grey, or yellowiſh, or blackiſh, the firſt of which is eſteemed the beſt. All ambergriſe, when kept for a certain time, is covered with a kind of white grey duſt like chocolate. When broken it appears to be of a granulated texture; and in ſome pieces it ſeems to be laid on in ſtrata.

It feels rather rough when firſt touched, but, when rubbed with the finger, it feels like hard ſoap, or rather like that kind of ſtone which the mineralogiſts call *Smectis*.

It is found ſwimming upon the ſea, or the ſea-coaſt, or in the ſand near the ſea-coaſt; eſpecially in the Atlantic Ocean, on the ſea-coaſt of Braſil, and that of Madagaſcar; on the coaſt of Africa, of the Eaſt Indies, China, Japan, and the Molucca Iſlands; but moſt of the ambergriſe which is brought to England comes from the Bahama Iſlands, from Providence, &c. where it is found on the coaſt. It is alſo ſometimes found in the abdomen of whales by the whale-fiſhermen, always in lumps of various ſhapes and ſizes, weighing from half an ounce to an hundred

and more pounds. The piece, which the Dutch East India Company bought from the King of Tydor, weighed 182 pounds. An American fisherman from Antigua found some years ago, about 52 leagues south-east from the Windward Islands, a piece of ambergrise in a whale, which weighed about 130 pounds, and sold for five hundred pounds sterling.

We are told by all writers on ambergrise, that sometimes claws and beaks of birds, feathers of birds, parts of vegetables, shells, fish, and bones of fish, are found in the middle of it, or variously mixed with it; but of a very large quantity of pieces which I have seen, and which I have carefully examined, I have found none that contained any such thing, though I do not deny, that such substances may sometimes be found in it; but the circumstance which to me seems to be the most remarkable, is, that in all the pieces of ambergrise of any considerable size, whether found on the sea, or in the whale, which I have seen, I have constantly found a considerable quantity of black spots, which, after the most careful examination, appear to be the beaks of the Sepia Octopodia. These beaks seem to be the substances which have hitherto been always mistaken for claws or beaks of birds, or for shells.

Having collected a pretty large quantity of them, I beg leave to present to the Royal Society some specimens, in which the whole structure of these beaks is extremely well preserved. They are accompanied by a beak which Sir JOSEPH BANKS permitted me to take from a cuttle fish in his collection, so that any gentleman, who will be at the pains to compare them together, will be enabled to convince himself of the truth of what I have advanced.

The presence of these beaks in ambergrise proves evidently, that all ambergrise containing them is in its origin, or must

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have been once, of a very ſoft or liquid nature, as otherwiſe thoſe beaks could not ſo conſtantly be intermixed with it throughout its whole ſubſtance.

In order to come now more cloſely to the point propoſed (viz. to determine the origin and nature of ambergriſe), let us recollect ſome of the principal facts relative to its natural hiſtory.

That ambergriſe is found either upon the ſea and ſea-coaſt, or in the bowels of whales, is a matter of fact, which, I believe, is univerſally credited. But it has never been examined into and determined, whether the ambergriſe found upon the ſea and ſea-coaſt is the ſame as that found in the whale, or whether they are different from one another? Whether that found on the ſea or ſea-coaſt has ſome properties, or conſtituent parts, which that found in the whale has not? And laſtly, Whether that found in the whale is ſuperior or inferior in its qualities and value to the former?

It is likewiſe a matter of conſequence to know, Whether ambergriſe is found in all kinds of whales, or only in a particular ſpecies of them? Whether it is conſtantly and always to be met with in thoſe animals? And, if ſo, in what part of their body it is to be found?

It is further a matter of enquiry, Whether, on thoſe coaſts where ambergriſe is found, there are alſo conſtantly, or only accidentally, whales to be met with? Whether ambergriſe is found there becauſe whales frequent thoſe ſeas, or rather whether whales are there becauſe ambergriſe is to be met with there?

It ought likewiſe to be inveſtigated, Whether all ambergriſe is of the ſame mineral or animal origin? If of the former, whether it is ſwallowed by the whale, and digeſted or changed in ſome manner in its ſtomach? Or, if of the latter, whether

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it is an animal production generated in the ſtomach as a kind of bezoar, according to CLUSIUS; or ſecreted in a peculiar bag, according to DUDLEY, &c.? Or laſtly, Whether it is, according to KÆMPFER, the excrement or dung of the whale?

All theſe queſtions ought to be diſcuſſed and preciſely anſwered, before we can determine any thing with certainty about the origin of ambergriſe.

In order to clear up this point, we muſt apply to the perſons who are employed in procuring and ſelling ambergriſe. This is what I have had an opportunity of doing through the kindneſs of Sir JOSEPH BANKS, baronet, whoſe zeal to promote every part of uſeful knowledge is ſo generally known and acknowledged by the public. Sir JOSEPH very obligingly procured me the acquaintance of two captains of ſhips, men of good ſenſe and veracity, who offered to tell me every thing they knew about the matter, and who began with aſſuring me that they would ſpeak only as to what they themſelves had ſeen, and that not once only, but repeatedly, as they have both of them been employed many years in what is called in England the South Fiſhery. I have ſince had an opportunity of converſing on the ſame ſubject with an intelligent native of New England, who before the preſent war broke out was employed for ſeveral years in the ſpermaceti-whale fiſhery from Boſton. From theſe three perſons I have collected the following facts:

Ambergriſe is ſometimes found in the belly of the whale, but in that particular ſpecies only which is called the ſpermaceti whale, and which from its deſcription and delineation appears to be the Phyſeter Macrocephalus Linnæi.

The New England fiſhermen, according to their account, have long known that ambergriſe is to be found in the ſpermaceti whale; and they are ſo convinced of this fact, that whenever

ever they hear of a place where ambergriſe is found, they always conclude that the ſeas in that part are frequented by this ſpecies of whale. It was for this reaſon that a gentleman at Boſton, upon hearing ſeveral years ago that ambergriſe was frequently found on the coaſt of Madagaſcar, immediately propoſed a plan for a ſpermaceti-whale fiſhery in that part of the world. And the two perſons I converſed with on this ſubject have not the leaſt doubt but he would have ſucceeded in the attempt, had not the Eaſt India Company fruſtrated the project, by pretending, that as it was in their territory the right of fiſhery could belong only to them. This was all they did, however, as the plan itſelf they never adopted.

The perſons who are employed in the ſpermaceti-whale fiſhery, confine their views to the Phyſeter Macrocephalus. They look for ambergriſe in all the ſpermaceti-whales they catch, but it ſeldom happens that they find any. Whenever they hook a ſpermaceti-whale, they obſerve, that it conſtantly not only vomits up whatever it has in its ſtomach, but alſo generally diſcharges its fæces at the ſame time; and if this latter circumſtance takes place, they are generally diſappointed in finding ambergriſe in its belly. But whenever they diſcover a ſpermaceti-whale, male or female, which ſeems torpid and ſickly, they are always pretty ſure to find ambergriſe, as the whale in this ſtate ſeldom voids its fæces upon being hooked. They likewiſe generally meet with it in the dead ſpermaceti-whales which they ſometimes find floating on the ſea. It is obſerved alſo, that the whale, in which they find ambergriſe, often has a morbid protuberance; or, as they expreſs it, a kind of gathering in the lower part of its belly, in which, if cut open, ambergriſe is found. It is obſerved, that all theſe whales, in whoſe bowels ambergriſe

bergriſe is found, ſeem not only torpid and ſick, but are alſo conſtantly leaner than others; ſo that, if we may judge from the conſtant union of theſe two circumſtances, it would ſeem that a larger collection of ambergriſe in the belly of the whale is a ſource of diſeaſe, and probably ſometimes the cauſe of its death. As ſoon as they hook a whale of this deſcription, torpid, ſickly, emaciated, or one that does not dung on being hooked, they immediately either cut up the above-mentioned protuberance, if there be any, or they rip open its bowels from the orifice of the anus, and find the ambergriſe, ſometimes in one ſometimes in different lumps of generally from three to twelve and more inches in diameter, and from one pound to twenty or thirty pounds in weight, at the diſtance of two, but moſt frequently of about ſix or ſeven feet from the anus, and never higher up in the inteſtinal canal, which, according to their deſcription, is, in all probability, the inteſtinum cœcum, hitherto miſtaken for a peculiar bag made by nature for the ſecretion and collection of this ſingular ſubſtance. That the part they cut open to come at the ambergriſe is no other than the inteſtinal canal is certain, becauſe they conſtantly begin their inciſion at the anus, and find the cavity every where filled with the fæces of the whale, which from their colour and ſmell it is impoſſible for them to miſtake. The ambergriſe found in the inteſtinal canal is not ſo hard as that which is found on the ſea or ſea-coaſt, but ſoon grows hard in the air: when firſt taken out it has nearly the ſame colour, and the ſame diſagreeable ſmell, though not ſo ſtrong, as the more liquid dung of the whale has; but, on expoſing it to the air, it by degrees not only grows greyiſh, and its ſurface is covered with a greyiſh duſt like old chocolate, but it alſo loſes its diſagreeable ſmell, and, when kept for a certain length of time, acquires the peculiar odour which is ſo agreeable to moſt people.

The gentlemen I converſed with confeſſed, that if they knew not from experience that ambergriſe thus found will in time acquire the above-mentioned qualities, they would by no means be able to diſtinguiſh ambergriſe from hard indurated fæces. This is ſo true, that whenever a whale voids its fæces upon being hooked, they look carefully to ſee if they cannot diſcover among the more liquid excrements (of which the whale diſcharges ſeveral barrels) ſome pieces floating on the ſea, of a more compact ſubſtance than the reſt; theſe they take up and waſh, knowing them to be ambergriſe.

From this account it appears therefore clearly, that CLUSIUS is quite wrong in aſſerting that ambergriſe is a phlegmatic recrement, or indurated undigeſtible part of the food collected and found in the ſtomach of the whale, in the ſame manner as the bezoars are found in the ſtomach of other animals. It appears further, that what DUDLEY ſays, in Phil. Tranſ. vol. XXIII. from an account he received from a whale-fiſherman, one Mr. ATKINS, of Boſton in New England, who was one of the firſt who went out a fiſhing for the ſpermaceti-whale about the year 1720, viz. that the ambergriſe found in whales is a kind of animal production like muſk and caſtoreum, &c. ſecreted and collected in a peculiar bag or bladder, which is furniſhed with an excretory duct or canal, the ſpout of which runs tapering into and through the length of the penis; and that this bag, which lies juſt over the teſticles, is almoſt full of a deep orange-coloured liquor, not quite ſo thick as oil, of the ſame ſmell as the balls of ambergriſe, which float and ſwim looſe in it; which colour and liquor may alſo be found in the canal of the penis; and that therefore ambergriſe is never to be found in any female, but in the male only, is equally deſtitute of truth. The aſſer-

tions are not only deſtitute of truth, but alſo contrary to the laws of the animal œconomy; for, in the firſt place, the gentlemen whom I conſulted have repeatedly found ambergriſe in males as well as females; they think, however, to have remarked, that the ambergriſe found in females is never in ſuch large pieces, or of ſo good a quality, as that which is found in males. 2dly, No man, who has the leaſt knowledge in anatomy and the animal œconomy, will ever believe that organiſed bodies, ſuch as the beaks of the Sepia, which are ſo conſtantly found in ambergriſe taken out of the whale, can have been abſorbed from the inteſtines by the lacteals or lymphatics, and collected with the ambergriſe in the bag mentioned by ATKINS and DUDLEY. If either of thoſe perſons had known the nature of theſe ſubſtances, and had had the leaſt knowledge of the different ſecretions in animal bodies, they would certainly never have ventured to give ſuch a deſcription as a true one.

KÆMPFER, who has given us ſo many other faithful accounts in Natural Hiſtory, ſeems to come nearer the truth with regard to the origin of ambergriſe, when he ſays, that it is the dung of the whale; and that the Japaneſe, for this reaſon, call it, Kuſura no fuu, *i. e.* Whale's Dung; but this relation, though founded on obſervation, has never obtained credit, and has been conſidered rather as a fabulous ſtory, with which the Japaneſe impoſed upon him, who had himſelf no direct obſervation to prove the fact.

This matter therefore remained a ſubject of great doubt, and it was generally thought to be more probable, that ambergriſe, after having been ſwallowed, and ſomehow or other changed in the ſtomach and bowels of the whale, was found among its excrements. But in order to diſcuſs this matter fully, and

bring it nearer to that degree of certainty which I propoſed at the beginning of this paper, it will now be proper to examine the principal queſtion, Whether all ambergriſe is generated in the bowels of the whale, or whether it is ſimply an extraneous ſubſtance taken in with the food? In order to elucidate this matter, it will be neceſſary to reſolve the following queſtions;

1ſt. Whether there is any material difference between ambergriſe found upon the ſea or ſea-coaſt, and that found in the bowels or among the dung of the whale, either with regard to its qualities and chemical principles, or with reſpect to the heterogeneous ſubſtances that are mixed with it? And 2dly, If there is any ſuch difference, in what does it conſiſt?

From the moſt exact information I have been able to procure on this ſubject, I find that what ſeveral authors have aſſerted, that all ambergriſe found in whales is of an inferior quality, and therefore much leſs in price, is deſtitute of truth. Ambergriſe is only valued for its purity, lightneſs, compactneſs, colour, and ſmell. There are pieces of ambergriſe found on different coaſts, which are of a very inferior quality, whereas there are often found pieces of it in whales of the firſt value; nay, ſeveral pieces found in the ſame whale, according to the above-mentioned qualities, are more or leſs valuable. All ambergriſe found in whales has at firſt when taken out of the inteſtines very near the ſame ſmell as the liquid excrements of that animal have; it has then alſo nearly the ſame blackiſh colour: they find it in the whale ſometimes quite hard, ſometimes rather ſoftiſh, but never ſo liquid as the natural fæces of that animal. And it is a matter of fact, that, after being taken out and kept in the air, all ambergriſe grows not only harder and whiter, but alſo loſes by degrees its ſmell, and aſſumes ſuch an agreeable one, as that in general has which is found

ſwimming upon the ſea; therefore the goodneſs of ambergriſe ſeems rather to depend on its age. By being accumulated after a certain length of time in the inteſtinal canal, it ſeems even then to become of a whiter colour, and leſs ponderous, and acquire its agreeable ſmell. The only reaſon why ambergriſe found floating on the ſea generally poſſeſſes the above-mentioned qualities in a ſuperior degree, is becauſe it is commonly older, and has been longer expoſed to the air. It is more frequently found in males than females; the pieces found in females are in general ſmaller, and thoſe found in males ſeem conſtantly to be larger and of a better quality, and therefore the high price in proportion to the ſize is not merely imaginary for the rarity-ſake, but in ſome reſpect well founded, becauſe ſuch large pieces appear to be of a greater age, and poſſeſs the above-mentioned qualities in general in a higher degree of perfection than ſmaller pieces.

Having diſcovered, as I juſt now mentioned, beaks of the cuttle fiſh in all the pieces of ambergriſe I had an opportunity of examining, it now remained to be aſcertained, how thoſe beaks became ſo conſtantly mixed with ambergriſe? In proſecuting this enquiry, I had the ſatisfaction to learn from the ſame perſons who gave me the information above-mentioned, that the Sepia Octopodia, or cuttle fiſh, is the conſtant and natural food of the ſpermaceti-whale, or Phyſeter Macrocephalus. Of this they are ſo well perſuaded, that whenever they diſcover any recent relics of it ſwimming on the ſea, they conclude that a whale of this kind is, or has been, in that part. Another circumſtance which corroborates this fact is, that the ſpermaceti-whale on being hooked generally vomits up ſome remains of the Sepia *.

From

* It will not be improper here to remark, to what an enormous ſize this ſpecies of Sepia grows in the ocean. One of the gentlemen who was ſo kind as to communicate

From what I have ſaid, we may eaſily account for the many beaks, or pieces of beaks, of the Sepia found in all ambergriſe.

The beak of the Sepia is a black horny ſubſtance, and therefore paſſes undigeſted through the ſtomach into the inteſtinal canal, where it is mixed with the fæces; after which it is either evacuated with them, or if theſe latter be preternaturally retained, forms concretions with them, which render the animal ſick and torpid, and produce an obſtipation, which ends either in an abſceſs of the abdomen, as has been frequently obſerved, or becomes fatal to the animal; whence in both the caſes, on the burſting of its belly, that hardened ſubſtance, known under the name of ambergriſe, is found ſwimming on the ſea, or thrown upon the coaſt.

From the preceding account, and my having conſtantly found the above-mentioned beaks of the Sepia in all pieces of ambergriſe of any conſiderable ſize, I think we may venture to conclude, that all ambergriſe is generated in the bowels of the Phyſeter Macrocephalus, or ſpermaceti-whale, and there mixed with the beaks of the Sepia Octopodia, which is the principal food of that whale; and we may therefore define ambergriſe to be the preternaturally hardened dung or fæces of the Phyſeter

municate to me his obſervations on this ſubject, about ten years ago hooked a ſpermaceti-whale that had in its mouth a large ſubſtance with which he was unacquainted, but which proved to be a dentaculum of the Sepia Octopodia, nearly 27 feet long: this dentaculum however did not ſeem to be entire, one end of it appearing in ſome meaſure corroded by digeſtion, ſo that in its natural ſtate it may have been a great deal longer. With regard to its being a dentaculum of the cuttle fiſh, the fiſhermen could not have been miſtaken, as they themſelves often feed upon the ſmaller ſort of the ſame Sepia. When we conſider the enormous bulk of the dentaculum of the Sepia here ſpoken of, we ſhall ceaſe to wonder at the common ſaying of the fiſhermen, that the cuttle-fiſh is the largeſt fiſh of the ocean.

Macrocephalus,

Macrocephalus, mixed with ſome indigeſtible relics of its food.

There now remains only one objection to be obviated on this ſubject, and this relates to the chemical analyſis of ambergriſe*.

NEUMANN obtained from one drachm of ambergriſe five grains of an acid phlegm, two ſcruples and an half of empyreumatic oil, and two grains of a volatile acid ſalt in a cryſtalline form.

Now if all ambergriſe owes its origin to the animal kingdom in the manner we have ſtated, how are we to account for the acid obtained from it by diſtillation? Would not ambergriſe, if it was really of an animal nature, like all other fæces of animals feeding upon animal food, yield a volatile alkali? I confeſs this ſeems to be a material objection; but I reply to it, firſt, that although my experiments made upon unadulterated ambergriſe confirm thoſe made by NEUMANN, GRIM, BROWNE, and GEOFFROY; yet from that analyſis it does, in my opinion, by no means follow, that ambergriſe is not an animal product.

Two eminent chemiſts, Mr. SCHEELE, and my friend Mr. BERGMAN, profeſſor of chemiſtry at Upſal, have lately diſcovered that human calculi of the bladder, though of an animal origin, are nothing elſe but a peculiar concrete acid, approaching in its qualities very nearly to the native vegetable acid: and Profeſſor CRELL has lately ſhewn, in a paper preſented to the Royal Society, that the preſence of an acid, far from proving any thing againſt an animal ſubſtance, is to be found in the fat

* Chemiſtry ſhews that in all animal excrements an acid is preſent, though different from that found in ambergriſe. Beſides, we do not know whether the marine acid of the ſea-water in which theſe animals conſtantly live, has not a ſhare in changing the nature of their fæces; nor whether the fæces of all cetaceous animals are perhaps by their chemical analyſis not materially different from thoſe of animals living on the Continent. We have a chemical analyſis of theſe latter, but none has been hitherto made of the former.

of

of all animals. This indeed proves as little as if I ſhould conclude on the oppoſite ſide of the queſtion, that becauſe the cruciform plants yield firſt a volatile alkali in diſtillation, they are of an animal nature. This, however, I have by repeated experiments with Cochlaria, Naſturtium, &c. ſeen to be conſtantly the caſe. With regard to the nature of the acid which is obtained by diſtillation from ambergriſe, nobody has hitherto to my knowledge examined it; and the experiments I made upon it are inſufficient to ſay any thing poſitive about it.

The great price of ambergriſe (an ounce of it being now ſold in London for one pound ſterling) has been hitherto the cauſe of its being ſo often adulterated, and of its being ſo little examined by chemiſts. If, however, a chemical analyſis of its acid ſhould be made, we ought to be certain that the ambergriſe employed has not been previouſly adulterated, eſpecially as it is but too common to find it adulterated with flower of rice, or with ſtyrax or other reſins, which might deceive us in forming a ſolid judgement about the real nature of its acid. The adulteration of ambergriſe with any of the heterogenous ſubſtances may be diſcovered by its not having all the qualities mentioned above as requiſite for the pureſt and beſt ambergriſe.

The uſe of ambergriſe in Europe is now nearly confined to perfumery, though it has formerly been recommended in phyſic by ſeveral eminent phyſicians; hence the Eſſentia Ambræ Hoffmanni, Tinctura Regia Cod. Pariſini, Trochiſci de Ambra Ph. Wurtemberg, &c. &c.

If we wiſh to ſee any medicinal effects from this ſubſtance, we muſt certainly not expect them from two or three grains, but give rather as many ſcruples of it for a doſe; though even then I ſhould not expect much effect from it, as I have taken of pure unadulterated ambergriſe in powder 30 grains at once, without

without obſerving the leaſt ſenſible effect from it. A ſailor, however, who had the curioſity to try the effect of recent ambergriſe upon himſelf, took half an ounce of it melted upon the fire, and found it a good purgative; which proves, that it is not quite an inert ſubſtance.

In Aſia and part of Africa ambergriſe is not only uſed as a medicine and as a perfume, but a great uſe is alſo made of it in cookery, by adding it to ſeveral diſhes as a ſpice; a great quantity of it is alſo conſtantly bought by the Pilgrims who travel to Mecca, probably to offer it there, and make uſe of it in fumigations, in the ſame manner as frankincenſe is uſed in Catholic countries. The Turks make uſe of it as an aphrodiſiac. Our perfumers add it to ſcented pillars, candles, balls or bottles, gloves, and hair-powder; and its eſſence is mixed with pomatums for the face and hands, either alone or mixed with muſk, &c. though its ſmell is to ſome perſons extremely offenſive.

Having now finiſhed my remarks about ambergriſe, I ſhall conclude this paper with ſome new obſervations concerning the ſebaceous ſubſtance generally called Spermaceti, and the whale from which it is obtained.

I mentioned above that it is only one kind of whale from which our fiſhermen obtain the ſpermaceti, which they call for this reaſon the Spermaceti Whale: in this ſame fiſh it is that they find ambergriſe. They never ſearch after the Phyſeter Catodon, the Phyſeter Microps, Phyſeter Turſio, and others of the ſame genus; but they aim at taking both the male and female of the Phyſeter Macrocephalus, though the male contains not only a larger quantity, but alſo in their opinion a better quality of ſpermaceti.

1ſt.

1ſt. It is to be obſerved, that this ſpecies has but one ſpout *(fiſtula)*. This ſpout is not, as hath been generally hitherto aſſerted, in the neck *(cervix)* of the fiſh, but in its front, and on the very edge of the head, bending obliquely on the left ſide, ſo that whenever he ſpouts it is always on that ſide only.

2dly. It is alſo remarkable, that the female of this whale has a power of drawing back its breaſts after it has ſuckled the calf, ſo that it hardly appears to have any prominence on the belly, whereas when it ſuckles they hang out very long.

3dly. It is not true, though it has hitherto been aſſerted, that the ſubſtance which we ſo abſurdly name Spermaceti, and which perhaps might with much greater propriety be called Sevum Phyſeteris, is found in the ventricles of the brain, and in the cavity of the ſpinal marrow of the Phyſeter Macrocephalus. This fat ſubſtance, which is nothing but a kind of ſuet, undoubtedly formed for ſome particular purpoſe of that whale, is contained in a peculiar bony triangular cavity or trunk, which is lodged near the brain, and occupies nearly the whole upper part of the head. This trunk has no communication with the brain, but is entirely ſeparated from it by its bony laminas. The brain, as in all other fiſhes, is very ſmall in compariſon with the ſize of the whale, and lies directly behind the eyes.

In order to know whether the trunk in which the ſpermaceti is lodged had any connection with the brain of the whale, one of the above-mentioned gentlemen had the curioſity to lance that trunk, which in its upper part is only covered with the ſkin, he found the whale not in the leaſt affected by this, but on the brain being lanced, the ſame whale died immediately.

XVI. *Extract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1782. *By* Thomas Barker, *Eſquire.*

Read March 13, 1783.

		Barometer.			Thermometer.						Rain.		
					In the Houſe.			Abroad.					
		Higheſt.	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean	Lyndon	Selbourn, Hamp.	South Lamb. Surry.
Jan.	Morn.	30,00	28,45	29,27	49	37	42½	49	21½	37½	2,333	4,64	2,23
	Aftern.				50	38	43	52	30½	41½			
Feb.	Morn.	29,99	28,72	29,50	48	33	37½	48	23	32	0,636	1,98	0,56
	Aftern.				49	34	38½	53	30½	38			
Mar.	Morn.	29,78	28,57	29,24	47	37	41	47½	23½	34	1,923	6,54	2,49
	Aftern.				49	38	42	54	37	44			
Apr.	Morn.	29,69	28,09	29,20	49½	41	44	45	30½	38	6,125	4,57	2,14
	Aftern.				50	42	45	55	38½	46			
May	Morn.	29,62	28,54	29,24	58	42	49	58	33	44½	5,722	6,34	4,10
	Aftern.				60	43	51	73½	42	55			
June	Morn.	30,05	29,06	29,60	68½	50	60	67½	44	55	1,295	1,75	0,49
	Aftern.				71	52	61½	82	52	66			
July	Morn.	29,82	29,10	29,51	68	57½	61½	62	50½	56	2,697	7,09	6,88
	Aftern.				69	59	62½	75	55	66½			
Aug	Morn.	29,64	28,60	29,21	62	55	59	57½	46	52½	3,114	8,28	
	Aftern.				63	56½	60	70½	55	63½			
Sept.	Morn.	29,89	28,73	29,47	62½	52	58	58	41	51	5,151	3,72	7,80
	Aftern.				64	53½	59½	67½	50½	61½			
Oct.	Morn.	29,86	28,80	29,40	52½	44½	48½	52	30	41	1,502	1,93	
	Aftern.				54	45	49½	59½	42½	50			
Nov.	Morn.	30,10	28,51	29,40	45	34½	39½	42½	23	32	1,074	2,51	1,24
	Aftern.				45	35	40	49	33	38½			
Dec.	Morn.	30,02	28,95	29,57	44½	34½	39	43½	25	34	0,517	0,91	0,72
	Aftern.				44½	34½	39	48½	30	38			
											32,089	50,26	28,65

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The beginning of January was chiefly mild, and ſome thunder; but as the year advanced it grew more froſty, intermixed with ſtorms and rain, and was a ſevere latter end of the winter; not long nor ſettled froſts, but frequent, eſpecially in March, which was almoſt all either froſt and ſnow, or ſtorms and wet, and was followed by ſo wet, cold, and backward a ſeaſon for two months as none remembered. Near twelve inches of rain in April and May, and every thing was ſix weeks or two months later than uſual, and the north and eaſt winds were wet, a ſure ſign of a wet ſeaſon; and ſometimes there came great rains from the eaſt for two or three days together, and vaſt floods.

The wall fruit was not only blaſted in the bloſſom, but moſt of what ſeemed ſet fell off afterward, and the leaves and ſhoots were ſo much killed, that the trees looked almoſt dead. The barley ſeed-time was very bad; a great deal could hardly be ſowed at all, or ſo late it was never well ripened. It appears to have been a very bad ſeaſon in other parts of Europe alſo. More froſt, and greater ſnows and rains than uſual, in the latter part of winter and ſpring, even in the ſouthern parts. In May and June there was an almoſt univerſal cold or other illneſs all over Europe, but few entirely eſcaped it; to many it was but ſlight, yet in ſome places it was mortal.

June was the beſt month this ſummer, the ſhowers being then fewer and ſmaller; yet there was never any long continuance of fine weather, but it was ſoon interrupted either by general wet fits, or by violent and great partial rains and thunder, in particular places, while it was fair elſewhere. In ſo wet a ſeaſon hay was, as it might be expected, plentiful, but a great deal of it ill got, and vaſt quantities of graſs the latter part of the ſummer.

The harveſt was very late and tedious. The laſt week in Auguſt, which was the beginning of harveſt, was pretty well, and the beginning of September very fine, only the mornings were miſty. In this time a good deal of the white corn was well got; but a great part of the barley was not then ripe, and the reſt of the harveſt, which in ſome places was not finiſhed till after October 20, was ſo wet, it was well it could be got in at all tolerably; yet from the coolneſs of the ſeaſon, and the unripeneſs of the barley, very little of it grew in this country or moſt others. The wheat was but a ſmall crop; the barley almoſt univerſally bad. The beſt crop was that of beans; but hardly any thing was well ripened, and all ſorts very dear, wheat three pounds a quarter, barley two pounds, oats one pound.

The three former years were pleaſant and fine, chiefly dry, and often hot, but by no means healthy. This, which ſeemed a very bad one for cold and wet, yet appears to have been more wholeſome. The latter part of the ſummer and the autumn, there has been much leſs illneſs about the country than for ſeveral years paſt.

A fortnight in the middle of October was tolerably fair for finiſhing the harveſt; then ſome ſtorms and rains. The end of October and beginning of November concluded the eight months wet ſeaſon, for the remainder of the year was dry. Almoſt all November was much inclined to froſt, and ſometimes ſevere, ſo as to threaten a hard winter. There was a ſmart froſt the latter end of the month, which continued, though with ſome breaks, above three weeks in November and December before it was quite gone; after which the laſt fortnight in the year was in general fine, calm, and mild: the ground

ground got quite dry in many places, and the wheat, which has lain a great while in the ground, comes up well at laſt.

An account of an uncommon circle ſeen about the Moon.

November 17, 1782, between ten and eleven at night, there was a remarkable corona about the moon, ſuch as I do not remember to have taken notice of before, at leaſt not to that degree.

It is common, when the moon appears through a thin cloud, to ſee a bright place round it, bounded by a yellowiſh red circle at a little diſtance, which ſeems to me to be not always of the ſame diameter. At this time the clear part of the ſky was very clear; but there were many thin clouds, and as they paſſed over the moon from the north, that uſual circle appeared much ſtronger than common, and I ſhould think of leſs diameter: but the remarkable part was, that round that circle, another rainbow-coloured one was ſeen; the blue, I think, was on the outſide, and the red terminated with the uſual red circle. The colours were far more diſtinct and bright than any halo, and not a third part of that diameter. It was brighter or fainter, according as different parts of the clouds paſt over the moon; and when the clear ſky came over it, the corona very nearly, if not wholly, diſappeared.

I have ſince ſeen ſome ſmall reſemblance of the ſame thing, but ſo faint I ſhould hardly have taken notice of it, if I had not ſeen it ſo much ſtronger at that time.

END OF PART I. OF VOL. LXXIII.

ground got quite dry in many places, and the wheat, which
has lain a great while in the ground, comes up well at last.

November 17, 1781, between twelve and eleven at night, there
was a remarkable corona about the moon, such as I do not
remember to have seen a ratio of before, at least not to that
degree.

It is common, when the moon appears through a thin cloud,
to see a larger place round it, bounded by a yellowish red circle
at a little distance, which seems to me to be always of the
same diameter. At this time the clear part of the sky was very
clear; but there were many thin clouds, and as they passed
over the moon from the north, that usual circle appeared
much stronger than common, and I should think of less dia-
meter: but the remarkable part was, that round that circle
another rainbow coloured one was seen by the blue. I think, was
on the outside, and the red terminated with the aforesaid circle.
The colours were far more distinct and bright than any iride
and not a third part of that diameter. It was another
moon, and when the clear sky came over it, the corona very
nearly, if not wholly, disappeared.

I have since seen some small reluminess of the same thing,
but to mine I should hardly have taken notice of it, if I had
not seen it so much stronger at that time.

END OF PART I. OF VOL. LXXIII.

APPENDIX.

Tranſlation of Count Franceſco Ippolito's *Letter to Sir* William Hamilton, *Knight of the Bath, F. R. S.; giving an Account of the Earthquake which happened in* Calabria, March 28, 1783. *See p.* 209.

THAT part of the kingdom of Naples, formerly poſſeſſed by the Brutii, and other Greek colonies, and now called Calabria, has been at all times expoſed to the terrible convulſions, of which we are at preſent the victims. The earthquakes in 1638 and 1659, by which the two provinces of Calabria were almoſt utterly deſtroyed, are freſh in every one's memory, as well as that of the year 1744, which afflicted us for a long time, but without loſs of cities or of men. Reggio, and the countries near it, are expoſed to earthquakes almoſt every year, and if we look back to higheſt antiquity, we ſhall find that all Italy, but particularly this country, and more particularly ſtill the provinces we inhabit, have been ſubject to various cataſtrophes in conſequence of volcanoes and ſubterraneous fires. Indeed, the religious rites themſelves of our anceſtors the Brutii, which hiſtory teaches us were all of a gloomy melancholy caſt, atteſt the deep impreſſion which the ſenſe of ſuch repeated and terrible cataſtrophes made upon the

people expoſed to them. Neither, however, could it, nor can it, be otherwiſe in countries ſuch as theſe are, which are interſected by the chain of the Appennines, the bowels of which contain nothing but ſulphur, iron, foſſil coals, petroleum, and other bituminous and combuſtible matters. The quantity of theſe minerals muſt neceſſarily occaſion fermentations and ſubterraneous fires, and it is good for us that we have ſo many volcanoes in the neighbourhood, to ſerve as chimnies, and afford outlets to the fire which forms under our feet.

But amongſt ſo many earthquakes to which we have been expoſed, the leaſt is not that under which we at preſent ſuffer, whether we conſider the force of the concuſſions, or their duration, or the changes that have taken place in the ſurface of the earth, or the ruin of ſo many cities and villages, with the loſs of forty thouſand inhabitants.

I have kept a regular account from the day of the firſt ſhock of the fifth of February, not only of the convulſions ſuffered by the earth, but likewiſe of all the meteors obſerved in the atmoſphere. This the ſhortneſs of time will not allow me to tranſmit to your excellency; but the ſum of it is, that from the 5th of February to this inſtant the ſhocks have been more frequent, and almoſt every day repeated. At times the earth ſhook as it uſually does on theſe occaſions; but at others the motion was undulatory, and at others vorticoſe, during which laſt ſtate it reſembled a ſhip toſſed about in a high ſea. The moſt conſiderable of theſe repeated earthquakes were thoſe which took place on the fifth of February, at 19¼ Italian time; on the ſeventh, about 20¼; on the twenty-eighth, about 8¼ of the night; and finally on the twenty-eighth of March, about 1¼ in the evening. Theſe four eruptions coming, as nearly as we can judge by the phenomena and effects, from the chain of mountains which extend from Reggio hitherwards, have produced four different exploſions in four different parts of Calabria. The three former were in that part of the province in which your excellency now is, and that which you muſt paſs through in your journey to Meſſina. Theſe exploſions have produced various great effects; ruined cities and villages, levelled

levelled mountains, immenſe breaks in the earth, new collections of waters, old rivulets ſunk in the earth and diſperſed, rivers ſtopped in their courſe, ſoils levelled, ſmall mountains which exiſted not before formed, plants rooted up, and carried to conſiderable diſtances from their firſt ſite, large portions of earth rolling about through conſiderable diſtricts, animals and men ſwallowed up by the earth—but I abſtain from entering into a minute account of theſe diſaſters; your excellency will ſee them with your own eyes, and aſſiſted by the relations of ocular and faithful witneſſes, no doubt, form a faithful hiſtory of them. One thing, however, I may not forbear to communicate, and that is, that of all theſe calamities the greateſt and moſt extraordinary was that which happened on the banks of Scilla and Bagnara. That part of the ſea which conſiderably overflowed in theſe marſhes, and ſwallowed up a great number of people who had taken refuge there, was ſo hot that it ſcalded ſeveral of thoſe who were ſaved. This I had from the mouth of the moſt excellent Vicar General.

But I will confine myſelf to a ſhort narrative of the effects of the laſt exploſion of the twenty-eighth of March, which, without a doubt, muſt have ariſen from an internal fire in the bowels of the earth in theſe parts, as it took place preciſely in the mountains which croſs the neck of our peninſula which is formed by the two rivers, the Lameto which runs into the gulph of St. Euphemia, and the Corace, which runs into the Ionian ſea, and properly into the bay of Squillace. That the thing was ſo is evident from all the phenomena.

This ſhock, like all the reſt, came to us in the direction of the S.W. At firſt the earth began to undulate, then it ſhook, and finally it moved in a vorticoſe direction, ſo that many perſons were not able to ſtand upon their feet. This terrible concuſſion laſted about ten ſeconds; it was ſucceeded by others which were leſs ſtrong, of leſs duration, and only undulatory, ſo that, during the whole night, and for half the next day,

the earth was continually ſhaken, at firſt every five minutes, afterwards every quarter of an hour.

A terrible groan from under ground preceded this convulſion, laſted as long as it did, and finally ended with an intenſe noiſe, like the thunder of a mine that takes effect. Theſe mighty thunderings accompanied not only the ſhocks of that night and of the ſucceeding day, but all the others which have taken place ſince that time: moreover, groans have ſometimes been heard without any ſhakes of the earth, and prior to the twenty-eighth of March there were noiſes and crackings which exactly reſembled the burſting of ſo many bombs.

The air was covered with clouds, and the weſterly gales blew very freſh. Theſe were ſtilled in one minute before the horrid craſh; but in one moment after they blew again, and then were ſtill. There were, however, frequent and ſudden changes of the atmoſphere during the whole night, the heavens being alternately cloudy and ſerene, and different winds blowing, though they all came from between ſouth-weſt.

At the time of the earthquake, during the night, flames were ſeen to iſſue from the ground in the neighbourhood of this city towards the ſea, where the exploſion extended, ſo that many countrymen ran away for fear; theſe flames iſſued exactly from a place where ſome days before an extraordinary heat had been perceived.

After the great concuſſion there appeared in the air, towards the eaſt, a whitiſh flame, in a ſlanting direction; it had the appearance of electric fire, and was ſeen for the ſpace of two hours.

In conſequence of the terrible ſhock, many countries and cities, eſpecially thoſe ſituated in the neighbourhood and neck of our peninſula as you go from Tiriolo to the river Angitola, and which had ſuffered nothing before, were overturned. Curinga, Maida, Cortale, Girifalco, Borgia, St. Floro, Settingiano, Marcellinara, Tiriolo, and other countries of leſs importance, were almoſt entirely deſtroyed, but with the loſs of very few people. Many hundreds, however, periſhed in Maida, Cortale, and Borgia.

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The ſame effects which took place in the country your excellency is now in were likewiſe produced by the earthquake in theſe parts. Many hills were divided or laid level; many apertures were made in the ſurface of the earth throughout the whole ſurface which lies between the two vallies occupied by the rivers Corace and Lameto, as you go towards Angitola. Out of many of theſe apertures a great quantity of water coming either from the ſubterraneous concentrations, or the rivers themſelves in the neighbourhood of which the ground broke up, ſpouted during ſeveral hours. From one of theſe openings in the territory of Borgia, diſtant about a mile from the ſea, there came out a large quantity of ſalt water, which imitated the motions of the ſea itſelf for ſeveral days. Warm water likewiſe iſſued from the apertures made in the plains of Maida; but I cannot ſay whether this was of a mineral quality, or heated by the ſame ſubterraneous fire.

We muſt likewiſe take notice, that there came from the ſame fiſſures out of which the water iſſued ſome very thin earth, either of a white, grey, or yellow ſort, which from its extreme tenuity had all the appearance of a true ſand. I have ſeen only the grey, in which there was evidently a mixture of iron.

It has alſo been obſerved, that in all the ſandy parts, where the exploſion took place, there were obſerved, from diſtance to diſtance, apertures in the form of an inverted cone, out of which likewiſe there came water. This ſeems to prove that from thence eſcaped a flake of electric fire. Fiſſures of this kind are particularly met with along the banks of the Lameto from the place where it goes into the ſea hitherwards for many a mile.

Amidſt the various phenomena, which either preceded or followed the earthquake, the two former are remarkable. On the very day of the earthquake the water of a well in Maida, which heretofore people uſed to drink, was infected with ſo diſguſtful a ſulphureous taſte, that it was impoſſible even to ſmell to it. On the other hand, at Catanzaro the water of a well, which before could not be uſed becauſe of a ſmell of calcination that

it had, became ſo pure as to be drunk extremely well. In Maida itſelf many fountains were dried up by the earthquake of the twenty-eighth. This likewiſe happened at other places; but many alſo broke out in ſeveral ſpots where there had been none before, as did alſo ſeveral mineral ſprings, of which before there was not a veſtige. This happened at Cropani, a country of the Marcheſato. Commonly, however, the fountains became more ſwelled and more copious, and emitted a larger volume of water than uſual.

The waters of ſome fountains were alſo obſerved to be troubled, and to aſſume a whitiſh or yellowiſh colour, according to the countries through which they paſſed.

Many elevations of ſoil likewiſe took place in conſequence of the earthquake. The moſt notable was that which happened in the bed of the river of Borgia, where there was ſeen a new hillock, about ten palms high, about twenty palms at the baſe, and about two hundred palms long. Finally, in the neighbourhood of the river Lameto, and preciſely in the diſtrict of the country called Amato, which was entirely torn up by the earthquake, there is an olive ground, the ſurface of which is turned over in a vorticoſe direction; a phenomenon which likewiſe obtained in many other parts of the country.

Such are the moſt notable phenomena of the earthquake of the twenty-eighth of March in theſe countries which have hitherto reached my notice. I think myſelf, however, obliged to notice to your excellency, that this extraordinary cataſtrophe of our afflicted province was preceded by great and extraordinary froſts in the winter of 1782; by an extraordinary drought and inſufferable heats in the ſpring of the ſame year; and by great, copious, and continued rains, which began in autumn, and continued to the end of January. Theſe rains were accompanied by no thunder or lightning, nor were any winds hardly ever heard in theſe cities where they are uſed to blow very freſh during all this time; but at the beginning of the earthquake they all ſeemed to break looſe again together, accompanied with hail and rain. For a long time before the earth ſhook, the ſea appeared conſiderably agitated, ſo as to frighten

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the fiſhermen from venturing upon it, without there being any viſible winds to make it ſo. Our volcanoes too, as I am confidently aſſured, emitted no eruptions for a conſiderable time before; but there was an eruption of Etna in the firſt earthquake, and Stromboli ſhewed ſome fire in the laſt. God grant that the pillars of the earth may be again faſtened, and the equilibrium of both natural and moral things reſtored!

I have the honour to be, &c.